letter on metric system p 1558
M. Thompson

SCIENCE

23 December 1960

Vol. 132, No. 3443

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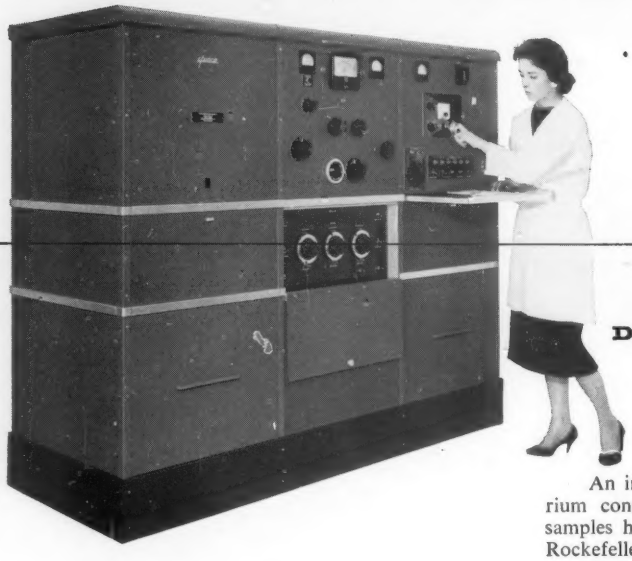

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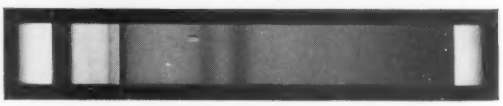

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The work appeared in the first issue of the German journal "Advances in Polymer Science"; reprints (in English) are available from Spinco.

If you are not familiar with the Ultracentrifuge, we will be happy to send you copies of "An Introduction to Ultracentrifuge Techniques" and the latest issue of "Fractions", a periodical sent to owners of Spinco ultracentrifuges, electrophoresis-diffusion instruments and amino acid analyzers. Write Beckman Instruments, Inc., Spinco Div., Stanford Industrial Park, Palo Alto 5, Calif.

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*American Journal of Clinical Pathology Vol. 33. No. 2. February 1960, pp 144-151 "Application of Refrigerated Microtome in Surgical Pathology" by Bernard Klionsky, M.D. and Othello D. Smith, M.D.

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Editorial The Disparagement of Statistical Evidence 1859

Articles A Biologist Examines the Mind and Behavior: *S. S. Kety* 1861
Many disciplines contribute to understanding human behavior, each with peculiar virtues and limitations.

Recent Statistical Studies in Astronomy: *T. Page* 1870
The nature of the galaxies and the birth-death process among comets are revealed in statistical studies.

Thurlow Christian Nelson, Marine Biologist: *M. R. Carriker* 1875

Science in the News Castro and Kennedy; Snow Pleads for More Scientists in Government 1876

Book Reviews H. H. Smythe and M. M. Smythe, *The New Nigerian Elite*, reviewed by *H. M. Miner*; other reviews 1882

Reports Blue Halite: *C. L. H. Howard* and *P. F. Kerr* 1886

An Inherited Male-Producing Factor in *Aedes aegypti*: *G. B. Craig, Jr., W. A. Hickey, R. C. VandeHey* 1887

Mechanism of Fibrillation Potentials in Denervated Mammalian Skeletal Muscle: *C.-L. Li* 1889

Preparation of Monolayer Cell Cultures from Tissues of Some Lower Vertebrates: *K. Wolf* et al. 1890

Hormology in Nutrition: *T. D. Luckey* and *P. C. Stone* 1891

Hemoglobin Types of the Caingang Indians of Brazil: *C. V. Tondo* and *F. M. Salzano* 1893

Appearance of a Substance in Acute Phase Serum Which Elicits Cx-Protein Responses: *R. F. Riley* and *Y. Hokama* 1894

Zinc-65 and Chromium-51 in Foods and People: *R. W. Perkins* et al. 1895

Calvacin: A New Antitumor Agent: *J. F. Roland* et al. 1897

Departments Letters from *M. B. Visscher* and *J. R. Baker; P. J. Hart; C. O. Houston, Jr.* 1858

Esperantists; Forthcoming Events 1899

Cover Salicylic acid crystal (about × 725). [L. C. Massopust, Marquette University School of Medicine]

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Letters

Vivisection Bill

I have seen the letter from Bradley T. Scheer of Eugene, Ore., [*Science* **132**, 851 (23 Sept. 1960)] dealing with S. 3570, a bill providing for federal regulation of animal experimentation. I am much distressed over what I think to be oversights in his reading of the bill. It will be found, if one looks at the wording carefully, that S. 3570 permits the use of live vertebrate animals "only" for medical and military research. No experimentation for the pursuit of knowledge per se is provided for, and no authorization is given for use of animals even in agricultural, veterinary, or animal husbandry work. Use of live vertebrate animals in teaching is authorized only for student surgeons, and then only if the animals are not allowed to survive the surgery—an absurd and crippling restriction. Scheer's statement that he "cannot find in this bill the evils" that others see is especially surprising because it would prohibit him from using live vertebrate animals unless he connects his work with medical or military objectives.

Scheer stated: "the bill gives no police powers to HEW or anyone else. . . ." Perhaps Scheer did not read section 4I which states, "Authorized representatives of the Secretary . . . shall be authorized to destroy or require the destruction of animals in accordance with rules, regulations, or instructions issued by the Secretary." What is this, if it is not police power?

The worst part of the bill, from my point of view, is that it would put in the hands of the Secretary of Health, Education, and Welfare complete power over the character of animal experimentation that could be performed in the United States under federal subsidy which now means most of such work. We are presumably (and hopefully) a country ruled by laws and not men. Dictators (even benevolent ones) are anathema to us. It is perhaps true, as Scheer says, that the Secretary of HEW is unlikely to put the most rigid interpretations possible upon his authority if S. 3570 became law, but why should the United States take the risk of some Secretary impeding scientific research by doing so?

On the other hand, I do wish to say that I think I can understand why so many well-meaning people are favorably inclined to the ostensibly mild, but actually very drastic, provisions in S. 3570. Such bills appeal to everyone's humane instincts and we, as biological scientists, should be careful to distinguish between the good motives and the lack of knowledge or poor judgment of the people who would like to satisfy their urge to promote gentleness in the use of experimental animals. Their lack of knowledge about the real situation may excuse many of them for their failure to recognize the great damage that would be done by the type of regulation they propose. Especially, they fail to see that, aside from providing more money for the construction and operation of facilities for the care of experimental animals, there is really no way in which federal intervention would actually increase the comfort of animals employed for legitimate purposes in scientific investigation and teaching.

MAURICE B. VISSCHER
Department of Physiology, Medical School, University of Minnesota, Minneapolis

In your editorial of 1 July 1960 you gave your reasons for opposing a bill (S. 3570), which, if enacted by the Senate and House of Representatives, would control vivisection in the U.S.A. In support of your opposition you made some quotations from a book written by myself. I recognize that you did so in good faith; but, to prevent misunderstanding, I want it to be known by your readers that I have studied this bill and hope that it will be enacted, for it has my full approval. I am a licensed vivisector under the laws of my own country.

JOHN R. BAKER
Department of Zoology and Comparative Anatomy, University Museum, Oxford, England

Conversion Factors

With reference to the letter from H. R. Dursch and the other letters published [*Science* **132**, 848 (23 Sept. 1960)] in reply to my letter [*Science* **132**, 256 (22 July 1960)], I am grateful to the various correspondents who called attention to my outdated tables of conversion factors. The observant Dursch, by the way, noted the revision of the nautical mile on 1 July 1954 but overlooked the revision of the length of the yard on 1 July 1959, a revision which increases the ratio nautical mile/statute mile from 1.150777 to 1.150779. (Incidentally, while replacing *his* outdated conversion tables, Dursch might also oil up his desk calculator and discover that the ratio 6076.1033/5280 does not equal 1.1507575).

Perhaps the various comments on my letter serve very well to emphasize the point I endeavored to make. Congratulations especially to William Allen who, having noted the recent revisions

(*Continued on page 1898*)

23 December 1960, Volume 132, Number 3443

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The Disparagement of Statistical Evidence

No one would claim that the theory of probability and that great body of statistical theory and practice which is based upon stochastic concepts are easy subjects, either technically or philosophically. The mathematical requirements are considerable, and the reasoning is often subtle.

But the basic procedures by which one treats the necessarily discordant results of any experiment or set of observations, and by which one calculates the degree of confidence justified by the combined result —these procedures are by now well developed, widely accepted, and competently known by any person with the requisite training.

Since the techniques of statistics are complicated, powerful, and not understood by the general public, there is always the chance of misuse. We remember with a shudder the clumsy enthusiasm with which correlation coefficients were seized upon, years ago, by many who wished to create an illusion of scholarly and scientific competence. And even today almost every big national magazine casts about to find a "statistical index" which "proves" that it has the most to offer to advertisers. It is not surprising that a clever and amusing book has been written under the title *How To Lie with Statistics*.

It must also be agreed that the statistical evidence which results from carefully designed experiments provides a much more solid foundation for inference than does statistical evidence which is, so to speak, merely "gathered."

All this is understandable. But it is shocking to note that various groups, in order to shake public confidence in statements which they find uncomfortable, are taking the position that it is silly to be impressed by evidence that is "only statistical."

For some time the outstanding offenders have been persons associated with the tobacco industry, who have claimed that the evidence for the relation between cigarette smoking and lung cancer is *only statistical*, as though that indicated a fancy and unreal sort of argument, which certainly would not affect down-to-earth persons.

But others are now taking similar attitudes towards statistical procedures. In a current news article I read that "the research directors of the Republican and Democratic campaigns say that . . . we find the statistics and then discount them."

It is, of course, possible that the polls in question were not competently planned, conducted, or interpreted. But it is essentially antiintellectual to indicate a blanket condemnation of statistical evidence.

Science recognizes the basic and the pervasive role played by probability and statistics. It is through a probabilistic procedure that every indvidual obtains the set of genes which to so great an extent determines what he is. The processes of communication, we have learned in recent years, are essentially statistical in nature. And on the broadest possible scale, our knowledge of the world about us is, in the present view, strictly and inevitably statistical in character.

For the individual behavior of every elementary particle in our universe is governed by laws which can only be expressed in probability terms. Everyday gross phenomena are normally predictable simply because of the vast numbers of individual events involved, the statistics thus becoming "regular" and dependable, just as is the experience of an exceedingly large life insurance company.

The automatic discarding of evidence because it is statistical is unscientific and wholly unwarranted. Statistical evidence is, in essentially all nontrivial cases, the only sort of evidence we can possibly have.—WARREN WEAVER, *Alfred P. Sloan Foundation, New York*

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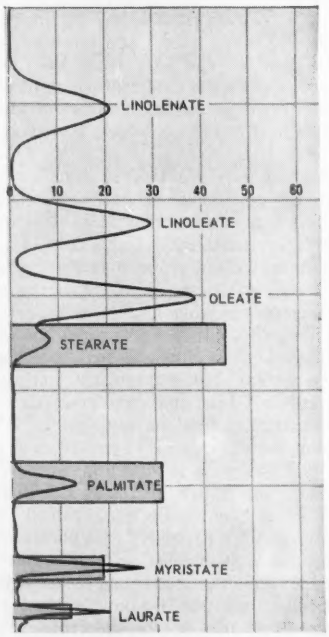

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23 December 1960, Volume 132, Number 3443 SCIENCE

CURRENT PROBLEMS IN RESEARCH

A Biologist Examines the Mind and Behavior

Many disciplines contribute to understanding human behavior, each with peculiar virtues and limitations.

Seymour S. Kety

I have given some thought to the question of whether, in an article such as this, it is better to present data or to discuss concepts, and have chosen to take the latter course. That is not entirely because the data are not so precise or so relevant as in some other fields; it is more because the knowledge of where to look, and how to look, and the meaning of what one finds may, in a field where the avenues to meaningful knowledge are uncertain or undiscovered, play a determining part in the productivity of men or movements.

It is no secret that psychiatry is such a field. There are well-ordered master plans in other branches of science, whose boulevards are already laid down, the trees pruned and the hedges clipped, and whose byways and alleys, even where they have not been broken through, have at least been indicated. In comparison with these, the territory of psychiatry is largely uncharted and unexplored, or spotted by primitive settlements trying to cut paths through the jungle between them. A creditable list of mental disorders have yielded their secrets to the pathologist or to the chemist, but the major psychoses taunt us today as they did the Hippocratic physicians.

It is easy to suppose, but difficult to demonstrate, that this state of affairs in psychiatry is to be ascribed to the ineptitude or indolence of its investigators, their lack of awareness of The Scientific Method, or their unwillingness to carry out research. A few direct encounters with some of the problems in the field are sufficient to convince one of the unparalleled intricacies of the nervous system and of the physical matrix which underlies behavior. If there is a more wonderfully complex structure in the universe, I do not know of it. One need not insist that psychiatry is not a branch of medicine to recognize that it is a superbly different one, nor deny the accomplishments and potentials of the physical sciences to recognize their limitations here.

Promise and Limitations of Biological Science

There are not many biological phenomena which tax the physicochemical complacency of the modern biochemist or biophysicist. The peculiar properties of protoplasm which formerly required the intervention of an *élan vital* seem almost comprehensible in terms of the versatilities of complex molecules; an understanding of the origin of life appears to have been made attainable through knowledge of viruses and bacteriophages which have filled the gulf between animate and inanimate matter, the deoxyribonucleic acid molecule appears capable of explaining genetic transmission, and concepts such as enzyme induction and biochemical specificity begin to provide models for the reduction of even the miracles of embryology to a self-determining series of physicochemical events.

Machines are being built or can be designed which will evaluate and discriminate, learn from experience, and adapt to changing situations. No matter how complex each segment of human behavior, an electronic circuit can be designed to duplicate it, so that even though the cost in resources and time would make such a construct unfeasible, the fact that physical models are conceivable does much to support what had previously been only a postulate of the mechanical basis of behavior.

The Problem of Consciousness

There remains one biological phenomenon, more central to psychiatry than to other fields, for which there is no valid physicochemical model and (or so it seems to me) little likelihood of developing one; this is the phenomenon of consciousness—the complex of present sensations and the memory of past experience which we call the mind.

When we look at the clear sky on a crisp autumn day, a remarkable sequence of physicochemical changes is set in motion, no less remarkable because it is commonplace. Today we can describe many of them, and we have every right to assume that some day we may be able to describe them all—from the light of a specific wavelength impinging on our retina, through the chemical and physical conversions there, to its emergence along the optic pathways as a series of specific signals in specific fibers. We shall trace these signals through the neuron pools in the

The author is chief of the Laboratory of Clinical Science, National Institute of Mental Health, Bethesda, Md. This article is adapted from the 12th National Institutes of Health lecture, delivered 18 May 1960 at the National Institutes of Health, Bethesda, Md.

great visual relay stations to certain portions of the visual cortex. We shall, I hope, someday be able to trace accompanying impulses through association pathways in the reticular system and in other areas of cortex, and if we are fortunate, we shall watch these ramifying impulses or their progeny converge in the motor centers of the brain in just the proper temporospatial arrangement to actuate the muscles which will say, "How blue the sky is today."

Where, pray, in that sequence is the sensation of blueness? It is neither wavelength, nor nerve impulse, nor spatial arrangement of impulses; it is not necessary to any of these processes and, though dependent on many of them, is explained or even described by none. It is richer and far more personal. One does not seem to get closer to its nature by increasing the complexity of its material counterpart —it is qualitatively and dimensionally different. As I indicated above, a machine can be built to perform any function that a man can perform in terms of behavior, computation, or discrimination. Shall we ever know, however, what components to add or what complexity of circuitry to introduce in order to make it *feel*?

These are not new thoughts, and they were not new to Aristotle or Plato, nor to Spinoza, Leibnitz, Berkeley, Hobbs, or Mach, nor to the other dozens of great minds which contemplated them. Modern students of the nervous system, with all their knowledge of the mechanisms which may underlie consciousness, have been unable to explain it any more than did the philosophers who preceded them; some (*1–4*) have, however, stated the problem quite cogently. Hughlings Jackson (*2*) wrote: "We cannot understand how any conceivable arrangement of any sort of matter can give us mental states of any kind. . . ." C. Judson Herrick (*2*) stated it more elegantly: ". . . awareness is an intrinsic psychobiological event, self centered and self contained. It is a product of a bodily mechanism, but it must not be identified with the mechanism that makes it. It has an identity which is distinctive and unique, an identity with qualities which cannot be described in terms of the temporospatial relations of the mechanism employed." Sir Russell Brain (*3*) has tried to come to grips with the problem by using the analogy of a pattern: "Not only are there twelve thousand million nerve cells out of which the patterns can be made, but nervous patterns exist in time, like a melody, as well as in space. If you look at a tapestry through a magnifying glass you will see the individual threads but not the pattern; if you stand away from it you will see the pattern but not the threads. My guess is that in the nervous system we are looking at the threads, while in the mind we perceive the patterns, and that one day we shall discover how the patterns are made of the threads." Fessard (*4*) very succinctly spots the difficulty: "Momentary distributions or patterns of excitatory or inhibitory state . . . has been proposed . . . as the basis for conscious experience; but what makes a pattern 'conscious' of its own patterning remains an irritating problem."

But science has faced other "irritating problems" without becoming paralyzed, and scientists have treated the problem of consciousness in one manner or another which is satisfying to them and permits them to get on with their work. There are the materialists and their psychological counterparts, the behaviorists, who solve the problem of explaining consciousness by ignoring it or denying that it exists. Their ritualistic avoidance of what they call "mentalisms" and their clumsy and inappropriate use of "behavior" in those instances when they could more meaningfully say "sensation," or "feeling," or "consciousness" make their approach less satisfying than it otherwise might be, but none can deny its usefulness.

By emphasizing the objective and measurable aspects of psychiatry and the behavioral sciences, they have demonstrated their kinship with medicine and the natural sciences and have brought into them considerable rigor at the price of just a little rigidity. But in denying the existence or the importance of mental states merely because they are difficult to measure or because they cannot be directly observed in others is needlessly to restrict the field of the mental sciences and to curtail the opportunities for the discovery of new relationships. The remarkable hallucinogenic properties of lysergic acid diethylamide (LSD) are barely hinted at in behavior, and the behavioral disturbances in schizophrenia are a mere fragment of the entire picture. Nature is an elusive quarry, and it is foolhardy to pursue her with one eye closed and one foot hobbled.

Then there are the idealists and the extremists among them—the solipsists, who are so struck with the fundamental and undeniable reality of consciousness that they are led to deny any other existence than their own sensations, for they cannot conceive of what the material world would be like aside from them. It is possible and valid to construct an astronomy with the earth central and stationary; Aristotle showed that it was possible, and Einstein's theories supported its validity.

In the same way, the doctrine that my own consciousness creates and determines the universe is pleasant, if pretentious, and quite unassailable. The real difficulty with egocentricity as with geocentricity is that it makes representation, comprehension, and prediction hopelessly complicated. The cosmogony, the physics, and the thermodynamics of a universe which is born each morning when I awake and annihilated when I fall asleep, which wobbles when I shake my head or have too much to drink, would make the complex equations of Ptolemaic astronomy a child's primer by comparison.

The glaring flaw in pure materialism and extreme idealism is that they are willing to entertain no inferences. And yet, science is born of inferences and thrives upon them. One may infer the existence of a universe of matter and energy outside of and quite independent of one's own consciousness and, by means of relatively simple generalizations called "laws of nature," render that universe and even consciousness itself more predictable and capable of description in parsimonious terms. One may also accept the direct and vivid testimony of the existence of one's own mental states and infer their existence in other beings similarly constructed.

Thus, one can acknowledge the existence of consciousness and of matter and energy without insisting that one must be reduced to the other. One can go further and study scientifically the relationships and correlations between them; one can without apology engage in a study of *psycho*pharmacology, first describing the effects of drugs not only on behavior but also on mental state and then attempting to elucidate their actions on the brain. One can, as Penfield (*5*) has done, study the particular sensations and mental states evoked from specific areas of the cortex; one can define a few of the correlates of sleep and attention in

terms of the electroencephalogram, or, as Evarts (*6*) is doing, in the activity of single cortical neurons. One can seek the anatomical and pathological basis of coma and find that the regions essential for the maintenance of consciousness seem to lie in the brain stem. One can determine the metabolic requirements and the energy equivalence of consciousness.

This last function—the energy equivalent of consciousness—was vigorously speculated upon and pursued by the early materialists in the belief that its evaluation would somehow demonstrate that consciousness was a chemical process which adhered to the law of the conservation of matter and energy. Between 1945 and 1948, we were in the position of being able to make such measurements in conjunction with measurement of cerebral blood flow in normal, conscious young men (*7*). From these data the rate of energy utilization could be computed. Twenty watts provide the total power for the human brain—for all of its physicochemical processes, all of its thinking, all of its consciousness. What about the dependence of consciousness on the brain's power supply? If, as the result of circulatory insufficiency, substrate deficits, or derangements in metabolism, this continuous supply of energy becomes attenuated, consciousness begins to fail; the difference between full normal consciousness and the depths of coma is only a matter of seven or eight watts (*8*).

Now that we have an energy equivalent for thought (and it is a maximum figure, since the relevant processes may constitute only a small fraction of the total) I am not at all sure that this proves the physical nature of consciousness; what it does do, for me at least, is to demonstrate all the more what a remarkable mechanism the human brain is, which can correlate, discriminate, compute, effect behavior, and feel with such a trivial expenditure of energy.

By this time the biologist within me becomes impatient and says, "Enough of this prattle about consciousness, which I grant you exists, but which I can't dissect without losing it, and which I can never hope to understand. These are problems I worried about and resolved to my own satisfaction years ago (*9*). Let us talk about behavior, which is essentially nothing more than the contraction of muscles and the secretion of glands. That is the area which my physics and chemistry will someday understand quite completely."

The Mechanistic View of Life and Behavior

Most modern biologists enter their laboratories each day with an implicit assumption that the phenomena they are about to study are physical and chemical in nature and bound by the same laws which describe the behavior of matter and energy generally. This mechanistic treatment of life has had a long history, going back at least to Democritus. It was proclaimed heresy by the ecclesiasticism of the Middle Ages, to be reborn in the reaction of the Renaissance and nourished and invigorated by the materialism of the 18th century and beyond. But nowhere is the doctrine expressed more clearly than in the writings of Claude Bernard (*10*), the progenitor of modern physiology.

"In living bodies as in inorganic bodies, laws are immutable and the phenomena governed by these laws are bound to the conditions on which they exist by a necessary and absolute determinism . . . determinism in the conditions of vital phenomena should be one of the axioms of experimenting physicians. If they are thoroughly imbued with the truth of this principle, they will exclude all supernatural intervention from their explanations; they will have unshaken faith in the idea that fixed laws govern biological science. . . . Determinism thus becomes the foundation of all scientific progress and criticism."

Although I share this faith, I cannot avoid pointing out that it is in fact faith rather than proof which forms the basis of this Olympian generalization.

Of course, there have been arguments against mechanism, the most recent being based upon Heisenberg's principle of indeterminism, which rests upon our inability to study the motion of the tiny particles of which the world is constituted without disturbing them and harks back to Lucretius, who, with Epicurus, visualized atoms veering from their determined course "in uncertain position and indefinite time, by an amount so small as cannot be expressed . . . from which veering alone can come that freedom which is potent to subvert the bonds of fate, which alone need not follow the chain of cause on cause eternal" (*11*). These arguments seem quite inapplicable. Those of Lucretius were based on the purest speculation, and even the Heisenberg principle, which is based upon the cold experience of modern physics, seems irrelevant. No mechanist ever expected to tally the position and motion of every particle in the universe, and being assured that one cannot do so is not much of a shock. It would seem that the concepts of freedom and purpose in the universe should be based upon nobler stuff than the clumsiness of our instruments.

The oldest argument against mechanism is the testimony of experience—we observe our will affecting our behavior in every moment of our waking lives. Against this the mechanist opposes a faith—but no demonstration—that the free choice was in fact an immutably determined event. Spinoza wrote (*12*), "Men think themselves free because they are conscious of their volitions and desires, but are ignorant of the causes by which they are led to wish and desire."

The modern mechanist can, moreover, correctly argue that no one has ventured to explain how a wish or desire in consciousness can move a muscle or activate a neuron. Clifford, who argued most convincingly for the reality of consciousness but also for its causal inefficacy, made this point (*13*): "the train of physical facts between the stimulus sent into . . . any one of our senses and the exertion which follows it and the train of physical facts which goes on in the train . . . these are perfectly complete physical trains and every step is fully accounted for by mechanical conditions. . . . If anybody says that the will influences matter, the statement is not untrue, it is nonsense. . . . It will be found excellent practice in the mental operations required by the doctrine to imagine a train, the forepart of which is an engine and three carriages linked with iron couplings, and the hindpart three other carriages linked with iron couplings: the bond between the two being made up of the feelings of amity subsisting between the stoker and the guard." And yet, even in classical Newtonian physics, we postulate and accept interactions for which there is no readily comprehensible model, explanation, or mechanism—for example, the gravitational "attraction" between two bodies across empty space.

Perhaps the most cogent argument

for a mechanistic concept of the universe, including animal and human behavior, is that it assumes and strives for predictability. And perhaps a need for predictability and a striving for it represent a primitive biological drive which the scientist in his laboratory shares with the young child or with the animal in the jungle, all of whom have learned that security in or mastery of their environments depends upon their ability to anticipate and predict its vicissitudes. Moreover, a mechanistic concept of the universe is heuristic. If productivity of a doctrine were the only proof necessary for its validity, the mechanistic concept would have been validated ten times over.

The mechanistic concept of behavior has sought and discovered a fascinating array of mechanisms which produce, determine, or modulate many aspects of behavior. The first two hundred years of the history of this search for a physiological basis of behavior was marked by a great debate over which was the organ of reason. The question appears to have been settled by the Hippocratic physicians who, around 400 B.C., wrote the final rebuttal of the Aristotelian notion that reason and feeling resided in the heart. I should like to quote their most succinct passage (*14*); even though it may have become quite familiar, its poetry and poignancy are nonetheless remarkable.

"And men should know that from nothing else but from the brain come joys, delights, laughter and jests, and sorrows, griefs, despondency and lamentations. And by this, in an especial manner, we acquire wisdom and knowledge, and see and hear and know what are foul and what are fair, what sweet and what unsavory. . . . And by the same organ we become mad and delirious and fears and terrors assail us, some by night and some by day, and dreams and untimely wanderings, and cares that are not suitable and ignorance of present circumstances, desuetude and unskillfulness. All these things we endure from the brain, when it is not healthy, but is more hot, more cold, more moist, or more dry than natural, or when it suffers any other preternatural and unusual affliction."

In the 23 centuries which have elapsed since those words were written, some progress has been made in postulating how the brain might perform certain of these functions and, to a more limited extent, in answering how in fact it does. Like the delusions of the paranoid psychotic, these designs for the brain have borrowed heavily from the technological developments of their times. We have seen first hydraulic, then mechanical, then crude electrical models of behavior. Cybernetics and computer and information theory, which in a sense emerged from efforts to understand the brain, have woven recently acquired anatomical and physiological information into a conceptual framework which electronics permits us to test in working models which perform useful functions.

Table 1. Cerebral oxygen consumption and mental state.

Condition	Cerebral oxygen consumption (% of normal)
Senile psychosis	82
Diabetic acidosis	82
Insulin hypoglycemia	79
Artificial hypothermia	67
Surgical anesthesia	64
Insulin coma	58
Diabetic coma	52
Alcoholic coma	49

Current Physiological Concepts of Behavior Mechanisms

Just as these electronic computers have tended to imitate the brain, so our concept of the nervous system has grown from what the computers have taught us. Instead of delineating a great sensory system and a great motor system feeding to and flowing from a massive memory and integrative system, one can view the nervous system as a decentralized organization of relatively autonomous functional units, each with its sensory, storage, and effector components and each with a built-in or acquired program.

Such systems could subserve practically all of our daily activities: standing, walking, shaving, driving our car, finding a parking space, pipetting, eating, playing the piano. A relatively small number, essential for immediate survival, such as breathing, sucking, and clasping, are probably laid down with the nervous system itself. Some are imprinted by early experience upon a genetically determined matrix, as exemplified in the lifelong positive tropism of the duckling for its mother but, equally, for any object which occupies its field of vision at a particular time after hatching. Some, such as sexual behavior, probably represent an acquired elaboration of an instinctive pattern. But many of the systems which mediate human behavior are experiential in origin, acquired by trial and error and by the persistence of the most successful or rewarding patterns.

Table 2. Cerebral oxygen consumption and mental state.

Condition	Cerebral oxygen consumption (% of normal or control)
Normal sleep	97
Schizophrenia	100
LSD psychosis	101
Mental arithmetic	102
Anxiety	118
Epinephrine infusion	122

There is remarkable economy in such a complex of semiautomatic sensory-motor programs to effect specific behavioral patterns. The cables actuating them can be relatively small, and the commands quite laconic, like "start; stop; slower; faster." These commands, in turn, can each be thought of as the result of a rigorous process of data reduction, whereby the tens of millions of impulses constantly pouring into the brain from all of the sensory receptors are sorted and compared against built-in or acquired master patterns to emerge as greatly condensed signals coded for destination and command or modulation.

These commands need not arise exclusively from the sensory systems; chemical changes arising from other regions of the body may operate at specific places to actuate appropriate sequences of behavior. Thus, dehydration sets off drinking by stimulation of specific osmoreceptors in the supraoptic nuclei of the hypothalamus, and certain of the sex steroids initiate appropriate sexual behavior in castrated animals when applied in infinitesimal amount directly to specific hypothalamic regions, and nowhere else. This study, reported only two years ago by Harris, Michael, and Scott (*15*), supports a remarkable speculation reached in 1905 by Freud (*16*): "We may now believe that in the interstitial tissues of the gonads special chemical substances are produced, which, when taken up in the blood stream, charge definite parts of the central nervous system with sexual tension."

There is a further economy in the multiple utilization of the same component in many circuits. A network with a finite number of interstices may still provide an almost infinite number

of separate pathways, and as the young brain is known to do, provide such pathways even after large segments are cut away.

Many have speculated that the cerebral cortex represents a great network where much of this storage, comparison, and coding occurs. The studies of Penfield (*5*), who, by stimulation of the human cortex, is able to elicit crude sensations, or sometimes highly integrated memories of past experience, are compatible with such a function.

To this model of the nervous system there have been added, in recent years, the neurophysiological counterparts of attention and affect. The central reticular formation, rescued by Moruzzi and Magoun (*17*) from the neglect which comes of ignorance, has been shown to have important relationships to sleep and wakefulness. This longitudinal network of short-branched interlacing neurons which taps the sensory systems and feeds into the motor outputs of other structures is strategically placed to intervene in the automaticity of the functional systems of which I spoke above. By selective facilitation, or by the inhibition of irrelevant activity, it can, so to speak, focus attention on a particular channel of sensory input or program of motor activity for the purpose of making and reinforcing new pathways. It appears that the anatomical substrate of consciousness resides here more than in any other place in the brain, not only because this seems reasonable but also because unconsciousness is most frequently associated with damage here. This remarkable ability to prevent the intrusion into consciousness of information irrelevant to the task in hand, which is so characteristic of alertness, appears to be accomplished through the reticular system not only by action at adjacent relay stations but also by newly elucidated sensory feedback systems which suppress the unwanted information at the peripheral receptors (*18*).

A system capable of representing affect or emotion is emerging from the imaginative speculations of Papez, substantiated by the functional studies of MacLean and the anatomical evidence of Nauta (*19*). The limbic system, corresponding to the rhinencephalon or olfactory brain, is richly connected with the reticular system, the neocortex, and, by way of the hypothalamus, modulates the endocrine and autonomic nervous systems. Specific areas of the hypothalamus which effect the release of the various trophic hormones of the pituitary have recently been elicited (*20*). Most interesting are the experiments of Olds (*21*) and of others which suggest the presence of centers for reward as well as aversive centers in the brain. Animals with electrodes chronically implanted in various limbic areas and connected to a lever which, when pressed, will deliver a slight electrical stimulus will press the lever at extremely high rates, continuously for many hours. For the reward of stimulating these special areas, animals will run a maze or traverse an electrified grid with greater alacrity than will a hungry animal going after food. It is not unreasonable to suppose that such areas of affect enter into the sensory and motor programming circuits to facilitate or inhibit their establishment, and to compete for attention in terms of value to the animal.

Growth and Flourishing of Neurochemistry

While the physics of the nervous system was being studied so productively by the neurophysiologists, an interest in its chemistry was developing, but much more slowly. Though derided by many of the powerful chemists of his day, Thudichum began a monumental program of extraction, purification, and analysis of the major chemical components of nervous tissue (*22*).

The next generation of neurochemists, influenced by the emphasis on cell respiration among the general biochemists, examined the energy metabolism of brain as exemplified by thin slices of that organ in a nutrient bath. Despite the liberties which they took with the functional integrity of their preparations, they learned a great deal about how the brain differed metabolically from muscle and liver: that the brain was the seat of a high metabolic rate; that its respiratory quotient was close to unity, indicating that carbohydrate was its main substrate for oxidation. These observations were later confirmed and extended in vivo, first in animals and finally in man. The latter studies, although they were, perforce, limited to the brain as a whole, had the unique advantage that all of the special features which characterize the function of that organ, including thought and consciousness, were preserved and could be correlated with cerebral oxygen consumption in normal man and in a variety of diseases.

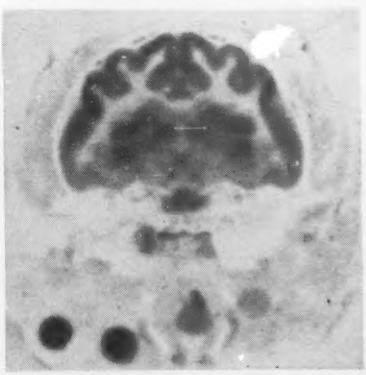

Fig. 1. Autoradiogram of a frozen section of the brain of a cat after exposure of the animal to a radioactive gas (CF_3I^{131}). There is a complex but roughly proportional relationship between photometric density and blood flow during life (as opposed to capillary blood volume) (*24*).

In Table 1 are presented some of the data of my associates and myself, as well as those of others, relating the over-all oxygen consumption of the brain to presumed mental state (*7*, *23*). There is a rough progression downward in terms of the degree of interference with function, and it is clear that this interference with function is correlated with cerebral oxygen consumption. It is likely that these conditions have in common a primary interference with energy transfer in the brain, either through a circulatory embarrassment, which we know occurs in senile psychosis from concomitant cerebral blood flow measurement, or through some metabolic blockade, as with hypoglycemic, and possibly diabetic, coma, or by suppression of synaptic transfer of activity, as may occur in anesthesia. There is another condition, anxiety, whether endogenous or associated with epinephrine infusion, in which a significant increase in oxygen and energy utilization occurs (Table 2).

But to me, the most interesting information contained in Table 2 is the finding that in a large group of mental states markedly different from normal there is no significant deviation in cerebral oxygen consumption. On the basis of the studies in sleep we were able to rule out the theory which attributed this state to the piling up of an unknown narcotic substance and to confirm certain neurophysiological interpretations. From the studies in schizophrenia we concluded that it requires just as much oxygen to think an irra-

tional thought as to think a rational one. But we also derived some understanding along one of the dimensions in which the brain is unique. "For it is neither a pump nor a motor and its current counterparts seem to be instruments of computation and communication. In such an instrument, although a defective power supply will produce dysfunction, meaningfulness of content and accuracy are by no means always correlated with the power used" (*8*).

In recent years neurochemistry has moved into the large and challenging area which lies between oxidation and the specialized functions of the brain, and into another dimension which distinguishes the brain from every other organ—its magnificent organization. Some of our own studies on cerebral blood flow and metabolism have also moved toward the regional differentiation of these functions (*24*). In Fig. 1 we see an autoradiogram of a cat's brain after brief exposure during life to a radioactive inert gas. The density of each area is a function of its cerebral blood flow, and from it the latter can be calculated. Since there is reason to believe that blood flow parallels metabolic demand, this gives us also an approximate map of the metabolic rates of the various parts of the brain during life. The differentiation, even on the basis of oxidative metabolism, is so marked that this figure is sometimes mistaken for a stained histological section.

But regional neurochemistry (*25*) has advanced far beyond oxidative metabolism. Lowry and his associates (*26*), by means of painstaking techniques by which it is possible to weigh individual neurons and analyze them for a variety of substrates and enzymes, are beginning to map the chemistry of the brain to its uttermost detail. In a number of laboratories a specific group of substances, the biogenic amines, have been demonstrated in relatively high concentrations and selectively distributed in the brain (*27*). The first of these to be studied was acetylcholine, and in recent years, in staccato fashion, we have learned about norepinephrine, histamine, γ-aminobutyric acid, and serotonin.

Scientists at the National Institutes of Health, such as Brodie, Udenfriend, Axelrod, and their respective associates, have contributed much to our understanding of the distribution, synthesis and degradation, and pharmacological interrelationships of these amines (*28*). The preferential distribution of some of them to the limbic system and the still crudely defined behavioral correlations of changes in their concentration in the brain suggest that they have important roles in behavior which remain to be defined. Udenfriend and his coworkers have shown that the enzyme for the synthesis of one of these amines, norepinephrine, exists in equal concentration in the caudate nucleus and in the adrenal medulla itself (*29*). It seems unlikely that these agents, pharmacologically active in other tissues, find themselves in the brain by sheer accident, and there is every reason to believe that neurochemistry, in association with precise psychological studies, will in the foreseeable future have a well-documented explanation of their presence in the brain.

The sensible and productive adolescence of neurochemistry portends a successful future, especially now that the earlier resistances of biochemistry to the field appear to be relaxing. That resistance was an interesting phenomenon and in marked contrast to the relationships of its parent discipline to neurophysiology. There were, of course, reasons for it.

The very origin of biochemistry from physiological chemistry—a handmaiden of clinical medicine—made it unduly defensive with respect to its interest in basic rather than applied research, forgetting all the while that biochemistry itself was an applied science, the application of chemistry to biology. But brain chemistry was looked upon as highly applied research—although I fail to see why the chemistry of neural transmission is less fundamental than the chemistry of fermentation or oxidation, or the chemistry of memory less interesting than chemical genetics. Then there was the doctrine of the "unity of biochemistry"—that all cells shared the same biochemical processes, so why gum up one's homogenizers with the mucky ointments of the brain when one could study bacteria. This doctrine, of course, emerged while biochemistry was preoccupied with the common mechanisms for energy production by cells—oxidation and phosphorylation—but it has always seemed to me like examining a tenement, the

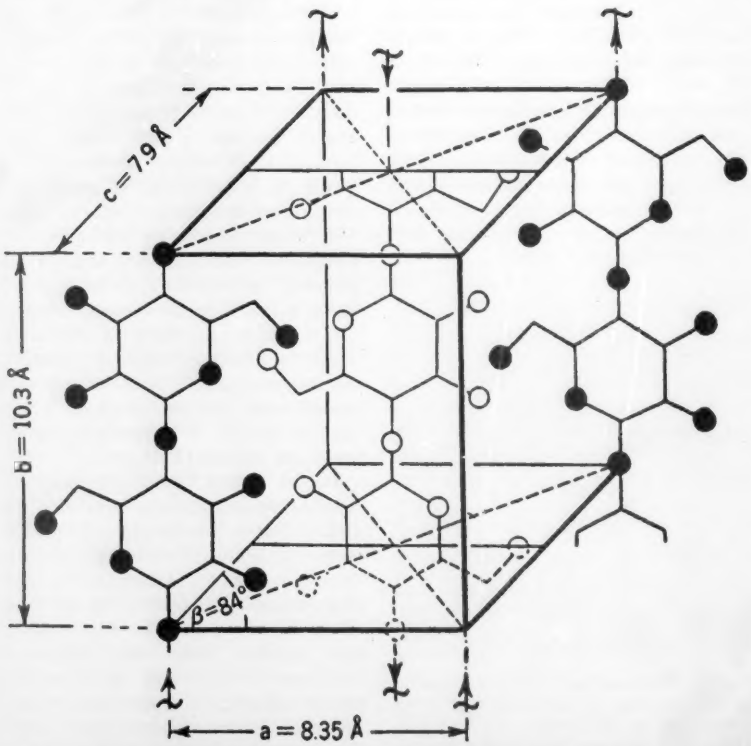

Fig. 2. A model of the molecular structure of cellulose. [Reproduced, with permission, from K. H. Meyer, *Natural and Synthetic High Polymers* (Interscience, New York, 1950), p. 304; copyright 1950 by Interscience Publishers, Inc., New York]

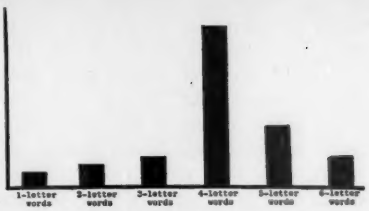

Fig. 3. Percentile frequency of words arranged according to their length in a particular book.

National Gallery of Art, and the White House, finding a furnace in the basement of each, and, without bothering to see what went on upstairs, declaring that they were all the same.

In a way this early provincialism served a useful purpose and perhaps contributed to the rich and rapid development of the field, and now that a point of diminishing returns appears to have been reached in the cataloging of enzymes and pathways of intermediary metabolism, biochemistry is moving upstairs, and the chemistry of differentiation and of genetics, chemical embryology, and even neurochemistry are becoming attractive areas. One can see a bright future in the study of the chemical processes in neural transmission—a work which has already begun; in the chemistry of memory, where interesting and heuristic hypotheses are being developed, centered on the coding possibilities which the protein molecule offers; in questions like neural specificity—how nerve fibers or neurons find their proper peripheral and central connections even when transplanted to unlikely places; and in the whole range of chemical processes in affect and behavior. For such work the biochemist will have to become interested in neurophysiology, in neuroanatomy, and in the behavioral sciences, and there is every indication that many are willing to do so.

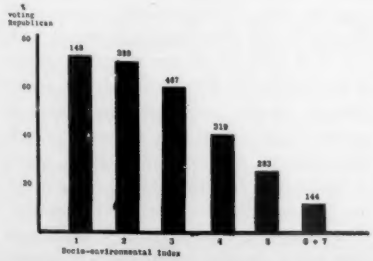

Fig. 4. Relationship between a socioenvironmental index and voting behavior. The figures above each bar represent total number in the sample. [From Lazarsfeld, Berelson, and Gaudet (*30*)]

So the sciences of biochemistry and biophysics have an assured and secure future in the study of the brain (and they were hardly waiting for me to point that out!). And if our assumption of the mechanistic nature of life and of behavior is correct, and man is nothing more than the most magnificent physicochemical engine which has ever been constructed, but an engine nevertheless, is it not obvious then that he or at least his behavior ought someday to be explained completely by physics and chemistry? If we believe that, and many do, then do not physics and chemistry and their sister biological sciences become the *real* sciences of behavior; while disciplines or bodies of knowledge and techniques like psychology, sociology, and psychoanalysis become merely empirical, descriptive, and derivative, to be tolerated as a sort of first-aid manual—what to do until the biophysicist or the biochemist arrives?

I should like to answer this question in the form of a parable; it is entitled "The True Nature of a Book."

The True Nature of a Book

Let us imagine a community with inhabitants who are of high intelligence and quite civilized except that they have never seen a book and have developed other means for the transmission of knowledge. One day a million books appear in their midst, an event which arouses so much curiosity and consternation that they decide to establish a scientific institute to study them. They set up this institute by disciplines and establish a policy that each scientist may examine these objects only with the tools and techniques and concepts of his discipline.

The first laboratory to be organized is the Laboratory of Anatomy. There the workers study these strange objects for a while, and their conclusion reads like this: "The specimen is a roughly rectangular block of material, covered ventrally and dorsally with two coarse, fibrous, encapsulated laminae approximately 3 millimeters thick. Between these lie several hundred white lamellae a fraction of a millimeter thick, all fastened at one end and mobile at the other. On closer inspection, these are found to contain a large number of black surface markings arranged in linear groupings in a highly complex manner."

By that time the chemists have appeared on the scene. The first chemist to get hold of a specimen burns it, and satisfies himself that it obeys the law of the conservation of matter and is therefore in his province; he may even compute the energy release per gram on complete oxidation. Next comes the analytical chemist, who discovers first its elementary composition but later breaks it down less completely into pure compounds; he also reports traces of elementary carbon, "which are probably impurities." Before I forget to mention it, one day a chemist accidentally drops a colored solution on one of the pages and by serendipity discovers paper chromatography, which lies around for 25 years before someone figures out what to do with it.

Then there are the biochemists, who slice the book and mince it and, best of all, homogenize it (because on the slices and the mince they can still see those black contaminants, while the homogenate can be centrifuged to remove them, permitting them to work with a Pure System). But all of these chemists have an uncomfortable feeling that though what they are doing is important, the real answers will come from the fellow down the hall who has just arrived and is still polishing his bright and expensive equipment—the molecular biologist.

With the self-confidence which comes from the adulation of the less fundamental sciences, he is anxious to begin work on the book he has selected because someone has told him that it is biased and distorted. Having hung a sign over his door which reads, "No twisted book without a twisted molecule," he proceeds to search for the molecule. By repeated extraction, centrifugation and ultracentrifugation, electrophoresis, hydrolysis, and repolymerization he finally isolates a pure substance, free of the carbon particles, and—what is even better—a macromolecule, and a twisted one at that. Figure 2 represents his version of the fundamental nature of the book, which many will recognize as a current hypothesis of the structure of cellulose.

Simultaneously, the physiologists have been attacking the subject. Unlike the biochemists, they have read the report from the anatomists and proceed to study and speculate upon why and how the pages are attached on one

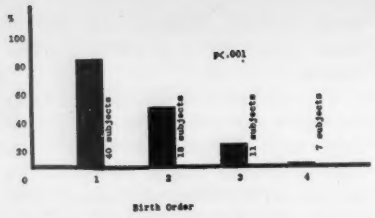

Fig. 5. Relationship between birth order and affiliative tendency under stress. [From S. Schachter (*31*)]

side. They study the movement of the pages as the book is riffled and derive complex equations to describe it. Then a biophysicist discovers that in an appropriate electrostatic field the graphite deposits produce discontinuities in potential. Fine microelectrodes are developed to pick these up, and amplifiers and oscilloscopes to display them. The biophysicists discover by sticking these electrodes into the book in various places that those which do not break off will pick up signals, some of which are reproducible. They develop thousands of tracings of these signals and call in the cyberneticist to help uncode them. The signals are recorded on miles of magnetic tape and fed into huge computers. Excitement mounts when, in a particular region extending over a few millimeters in a certain book, one of them discovers on a particular day that, for a few minutes before he damaged the source of the signals, a tremendously complex pattern appeared which was reproducible but incomprehensible. This pattern is fed into the data reducers and the computers, which can generate and test thousands of hypotheses per minute. Finally, the electric typewriter begins to print; a meaning has been found in that complex pattern—it reads "THE."

By this time the behavioral scientists have been admitted to the institute and begin to study the problem. They are a strange lot. Some of them have read the reports of the anatomists, the chemists, and the physiologists, but many of them don't seem to care. Most will admit, if pressed, that the book is material in nature, that it obeys material laws, that it and its contents are nothing more than a highly specialized arrangement of chemical substances. But they don't slice the book, and they don't purify its chemical components—in fact, they seem to feel that it is improper to do so. Instead, they ask questions peculiar to their disciplines and look in the book for the answers. The first one likes to count, so he counts the number of letters in the words and comes up with a frequency distribution of the words by their length (Fig. 3). He finds a preponderance of four-letter words, forms a hypothesis that the book is a modern novel, and ventures a prediction that it will be a best seller and also banned by the Postmaster General. Then he looks for particular words and counts them and confirms the hypothesis. His colleagues join him, asking other general questions and finding their answers in the content of the book. They learn a great deal about classes of books, how they differ from one another, and what their effects are on the community. Although the behavioral scientist has learned much about the nature of books—infinitely more, in certain areas, than the physical scientist—his techniques falter in the area of the individual book, its characteristics, and his ability to make entirely reliable predictions about it. If it is important to learn something about the individual book, then there is need for a technique which can read it completely. Such a technique has not yet appeared, but some progress has been made in its development.

Finally, the book is brought in desperation to the psychoanalyst in the hope that he will be able to read it. That he does not do precisely, but instead asks the author to select portions and read them while he listens. Of course, the author is biased and reads what he wants to read, or, if there is "good transference," those passages which he thinks the analyst would like to hear. And the analyst himself doesn't always hear with equal acuity but, depending on his school or on his preconceived notions, is deaf to greater or lesser portions of the data.

Nonetheless, this anecdotal, biased, and selected patchwork may be the closest approximation which we have to the rich and almost inexhaustible fund of information which reposes in the individual human brain and, to a significant extent, determines individual behavior. Like all scientific methodologies this one was not born perfect and complete, and there are increasing numbers of analysts who recognize that. Many of the unavoidable biases in the data may not be all bad. To deal with all of the stored information would be impossible; some selection is clearly necessary, and there is some chance that the selection which the subject employs may have some relationship to the actual weightings of the data in his affect and behavior. Furthermore, this particular technique has been largely employed for therapeutic purposes, and clinical therapy in other branches of medicine has not always been characterized by the strictest adherence to scientific methodolo-

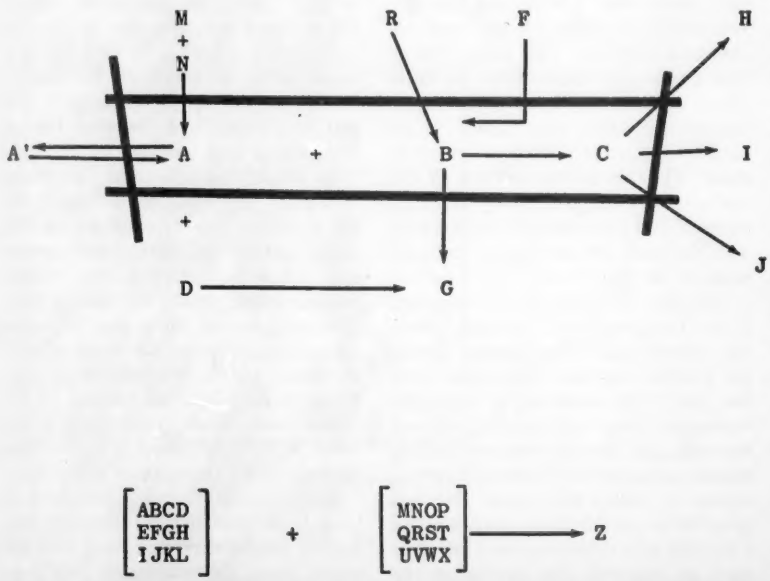

Fig. 6. Models of problems typical of those encountered (top) in biochemistry and (bottom) in human behavior.

gies. There is increasing recognition of its unique values and limitations as an instrument of research, and its critical use in that connection by trained and qualified observers is a worth-while goal toward which perceptible progress is being made.

The Hierarchy of the Scientific Disciplines

If I seem to have attached increasing values to the disciplines as I have enumerated them, I have done so only to counteract a hierarchical tendency in the opposite direction which I fear exists today. There are no higher or lower, better or worse, disciplines except with respect to their relevance to particular problems.

In the case of the brain, the biological disciplines have made and will continue to make remarkable progress toward understanding its structure, its metabolism, its functional interrelationships, and the mechanisms which underlie behavior, and they have solved or will solve those mental disorders which are primarily the result of disturbances there. But in the area of information, content, and experience, stored as it is in the complex interrelationships of 13 billion neurons, biology is extremely pretentious if it thinks that it can unravel them by means of its tools. There will, no doubt, some day be a biochemistry or a biophysics of memory—but not of memories.

Take the question of voting Republican or Democratic in a particular election. All of the experience and the biases and the motivation for doing one or the other are stored in the physical chemistry of the brain, but these cannot be reached by physics or chemistry. There are other, more appropriate, techniques for that. Figure 4 depicts a correlation with voting behavior of an index based on only a few factors in the experience of individuals: economic level, religion, and urban versus rural abode (*30*). In Fig. 5 the tendency of individuals to want to be together or alone in an anxious situation is correlated with a single factor, birth order, and a surprisingly large segment of the variance is defined (*31*). Information like that is obtained without attempting to get to the fundamental physiological basis of the tendency; in fact, it would be lost if we were to try.

In Fig. 6 I have attempted to represent two kinds of problems which face scientists. One (Fig. 6, top) seems deceptively complex but is relatively simple, since by four cuts one can rule out all but the relevant variables and study a fundamental process: *A* plus *B* yields *C*. That is the kind of problem to which biochemistry has frequently addressed itself and which it has solved so successfully.

But even in biochemistry, that approach is not the one always and uniquely adapted to yield truth, as DeWitt Stetten (*32*) has so keenly pointed out: "Which of these many levels of disorganization is then to be recommended? The answer to this question is that unequivocal results as to what is happening in the intact mammal cannot be gleaned from studies conducted at any one level. Each level, considered separately, has led to unsatisfactory conclusions. We know of reactions catalyzed by isolated enzymes for which no counterpart in the intact animal has been discovered. We know of over-all conversions observed in the intact animal which have thus far defied study at subcellular levels and for which no enzymes have been unearthed. It is improper to hold that any one approach is in all cases superior to all others. Selection of level of disorganization, often in fact determined by the skills or prejudices of the individual investigator, should be based upon the nature of the specific question which is being asked." If this is true for metabolism, how much more true is it for behavior?

In Fig. 6 (bottom) one sees a representation, deceptively simple but actually terribly complex, of what happens in behavior—say, an interaction between a candidate, or the impression of a candidate, and a voter that results in a vote. *A*, *B*, *C*, and *D* are certain obvious characteristics — the candidate's party, his stand, his age, among others. *M*, *N*, *O*, and *P* are obvious characteristics of the voter—his economic class, his religion, his income, and so forth. All the other factors (and there are many) represented by the other letters, and many more which are unrepresented, are the idiosyncratic ones—the candidate's smile, how he parts his hair, how the voter feels toward his father, what books he has read, what people he has met, to list a few. The net reaction is the resultant of *all* of these and is quite a different one for each case, even though the outcome is restricted to a choice between *Y* and *Z*. It is the problem of the sciences of behavior to develop techniques for the study of multivariant processes without reducing them to simpler ones which do not ask or answer the particular question. Let us hope that such techniques will continue to be developed and be intelligently used, for much depends upon them.

It has most certainly not been my intention to deny the tremendous importance and the major contributions which biochemistry and biophysics and the biological sciences generally have achieved within our lifetime. I have merely wanted to point out that we do not always get closer to the truth as we slice and homogenize and isolate—that what we gain in precision and in the rigorous control of variables we sometimes lose in relevance to normal function, and that, in the case of certain diseases or problems, the fundamental process may often be lost in the cutting. A Heifitz and a Rubinstein playing different sonatas at the same time will produce a cacophony which the most exhaustive study of either individually would never have revealed, and a truer picture of the nervous system and behavior will emerge only from its study by a variety of disciplines and techniques, each with its own virtues and its own peculiar limitations (*8*).

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Recent Statistical Studies in Astronomy

The nature of the galaxies and the birth-death process among comets are revealed in statistical studies.

Thornton Page

There have been many advances in statistical astronomy since Herschel first reasoned from the concentration of stars in the Milky Way that the sun must be located in a disk-shaped system of stars now called the Milky Way galaxy. Later studies of the distances and motions of stars have proved well suited to statistical analysis, and more recently the analogous studies of galaxies have been added to this list. It was therefore appropriate that a portion of the Fourth Berkeley Symposium on Mathematical Statistics and Probability, held at the University of California from 20 June to 29 July, 1960, should be devoted to recent applications of statistics in astronomy. The problems discussed were in the fields of radio astronomy, dynamics, and cosmology, and they concerned both the most massive and the least massive of astronomical bodies: galaxies and comets.

Distances of Cosmic Radio Sources

The most recent additions to the variety of celestial objects studied by astronomers are the strong sources of radio waves from outside the earth. When radio telescopes first came into use it was found that much of the cosmic radio emission comes from our Milky Way galaxy, or from objects belonging to it, such as the Crab nebula (a cloud of turbulent gas remaining from the explosion of a star several centuries ago) or the sun itself. However, there are about 100 small regions of apparently empty sky from which strong radio signals are also coming—signals which could not at first be identified with any visible object but which are now known to be from extragalactic sources located far outside our galaxy. Two questions then arose: How far away are these extragalactic radio sources, and, if we built even more powerful radio telescopes, how many more such sources would be detected?

R. Minkowski of the Mount Wilson and Palomar Observatories, Carnegie Institution of Washington, and California Institute of Technology has identified many of the extragalactic radio sources with distant galaxies found on photographs taken with the 200-inch telescope and other large optical instruments. Over the past five years he has obtained a sufficient number of these identifications to study the radio sources statistically.

Since the radio observations themselves offer no direct evidence of the distance of the source, Minkowski obtained optical spectra of 17 of the faint galaxies identified with the nearer radio sources; from these he measured the red shift or apparent velocity of recession, and from the Hubble law (that the red shift is proportional to distance) computed distances ranging from 30 million to 3000 million light years. Though they appear faint, these radio-emitting galaxies have an average optical brightness 25 billion times that of the sun and 3 or 4 times that of a normal galaxy, when the distance is taken into account. In the same manner, their individual outputs of radio power can be determined; these are as large as 5×10^{29} watts per cycle per second at a radio frequency of 158 megacycles per second.

Comparing the numbers of radio sources found in surveys of over half the sky with data from comparable

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surveys of ordinary galaxies, Minkowski has found that less than 1 percent of the ordinary galaxies nearby emit as much as 5×10^{16} watts per cycle per second, less than 0.005 percent emit 5×10^{18} watts, less than 0.000025 percent emit 5×10^{20} watts, and so on. However, the radio-survey counts (Fig. 1) increase so rapidly as fainter and fainter sources are counted that the proportion of radio emitters is much higher in very distant galaxies that are too faint to be photographed, even with the 200-inch telescope.

This enigma—that the density of radio sources seems to increase with distance from us on all sides—may be explained in terms of some evolutionary effect; at the great distances involved—billions of light years for the strongest sources—we are observing galaxies as they were billions of years ago, and at that time the mechanism of radio emission from galaxies may have been more prevalent.

The mechanism of radio emission is itself an enigma; Minkowski's photographs show that some of the stronger, identified (nearer) radio sources are close pairs of galaxies, apparently in collision; others appear peculiar in form, some with faint, jetlike structures. From the nature of the radio emission it is clear that the source must be ionized gas in violent motion, such as might occur in a collision between gas clouds at several hundred miles per second or in a very large release of energy from a sudden nuclear explosion. After an interval of time these mass motions would be expected to damp out, heating up the gas. The implication is that collisions between galaxies, or vast nuclear explosions in huge unstable stars, must have been more frequent in the past than they are today. (Minkowski used the Einstein–de Sitter model of the universe, which takes into account [Fig. 1, solid curve] another factor affecting the apparent density—namely, that since all the galaxies are receding from us and from each other, the general density would have been much larger billions of years ago.)

The enormous distances of extragalactic radio sources, suspected for the past few years, can now be determined quantitatively by Minkowski's analysis, confirming the belief that the radio telescope is the most powerful tool for probing the farthest reaches of space and time.

Average Mass of a Galaxy

In our galaxy there are many billions of stars, some larger than the sun, some smaller. It would be impossible to count them, but the total mass can be estimated quite simply from the motion of stars near the edge under the gravitational attraction of the whole; the total mass is thus determined at several hundred billion times the mass of the sun—more precisely, at 2×10^{11} suns. A few nearby galaxies can be "weighed" in the same manner, by measuring shifts in the spectra of their outer parts. These masses are found to vary widely, from 10^9 to 10^{13} suns.

The average mass of a galaxy is of importance in relativistic cosmology for determining the average density of matter in space, a parameter that is expected from Einstein's theory to affect geometry through the curvature of space. One attempt to determine this average mass involved study of random motions of galaxies in clusters. The mass of the cluster acts by gravitation on the individual galaxies in much the same way that each galaxy acts on its

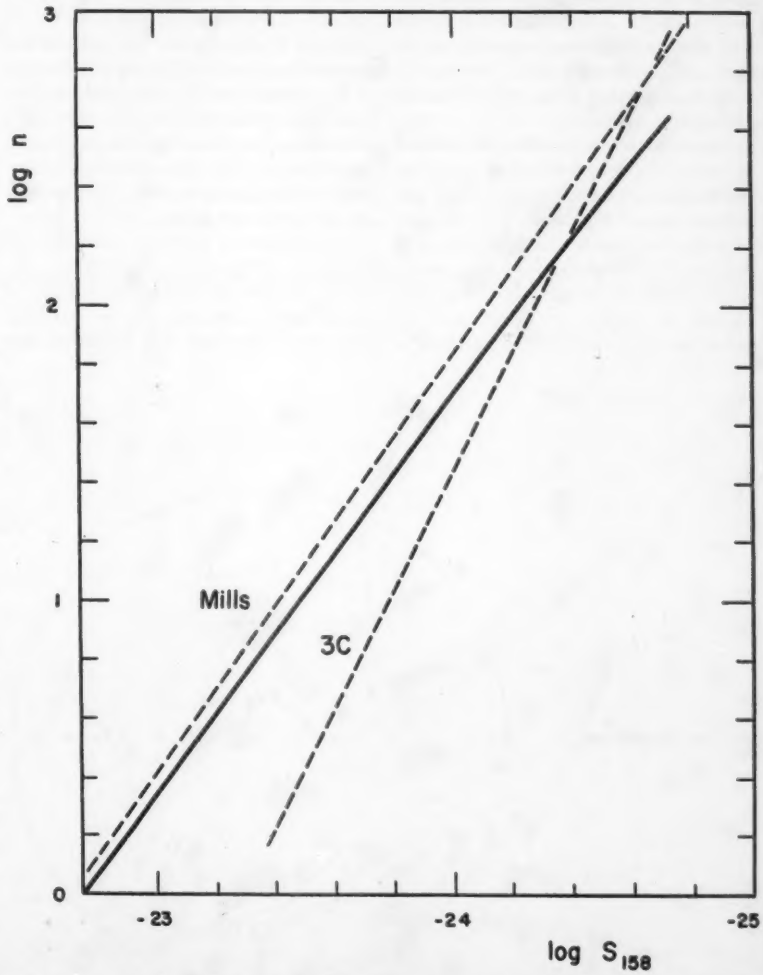

Fig. 1. Number of extragalactic radio sources brighter than flux density S_{158} (measured in watts per square meter per cycle per second, at frequency of 158 megacycles per second); n is the number over the whole sky, corrected for the smaller area actually surveyed. The dashed curves represent the observations of Mills *et al.* (*3*) and Edge *et al.* (*4*). Radio observers now agree that correct observations lie along a line that approximately bisects the two dashed lines, a good deal steeper than the full curve derived by Minkowski from optical counts on the assumption that a constant proportion of galaxies are radio sources.

component stars. However, this method yields an average mass of over 10^{12} suns—100 times the median mass of nearby galaxies.

In an effort to resolve the matter and to learn more about differences in mass between galaxies of different types, I measured motions in pairs of galaxies. In each pair, the galaxies appear to be moving round one another just as the earth moves round the sun, and the total mass of each system is simply related to the size of the orbit and the period. The masses and orbit sizes are such that the periods are 200 million years or more. Moreover, the orbit plane is oriented at some unknown angle, and the line between the two galaxies is oriented at yet another unknown angle; hence I could measure only the instantaneous projected separation and projected orbital motion in each pair, getting a "snapshot" of the full orbital motion.

However, for a considerable number of pairs it can be assumed that the orientation is random—that is, the average mass of galaxies in a number of pairs can be determined statistically. Because the measurements of velocity from small-scale spectra of these faint objects are subject to error, and because the distances between galaxies in a pair are not all the same, the statistical analysis is fairly complex. The theory is based on the assumption that the orbits are circular and leads to the prediction that the regression of projected orbital velocity on the reciprocal of the apparent separation is proportional to the average mass.

I applied this result to my own measures and to others made by Humason at the Mount Wilson Observatory and by Mayall at the Lick Observatory, for a total of 52 pairs of galaxies or groups of galaxies, obtaining an average mass of 3×10^{11} suns with a probable error of $\pm$ 20 percent. For 14 pairs of galaxies of the spiral type, the average mass is only 0.3×10^{11} suns ($\pm$ 70 percent), and for 18 pairs of the elliptical type, $6.5 \times 10^{11} \pm 30$ percent. It thus appears that the average mass of a galaxy in this sample depends very strongly on the type, and the fact that the spiral galaxies are only 1/20 as massive as the elliptical galaxies is inconsistent with the evolutionist conception that spirals evolve into ellipticals in the course of time.

In connection with the evolution and make-up of galaxies themselves, the ratio of mass to luminosity (M/L) is important. Using the sun as standard ($M/L=1$), we find that the large, hot blue stars have small mass for their luminosity, whereas cool, red stars generally have large M/L. Modifying the statistical theory to include measured brightnesses of the galaxies, I determined M/L to be 0.3 ± 70 percent for spirals, an indication that they consist almost entirely of hot blue stars, and 100 ± 30 percent for ellipticals, an indication that they consist of very cool stars (inefficient radiators) or an admixture of nonluminous material.

Since this method of mass determination includes all the matter within the orbit of each pair of galaxies (a sphere of some 150,000 light years' diameter), it is possible that the resulting masses include a portion of intergalactic matter; however, the intergalactic density would have to be 10^{-27} gm/cm^3 to affect the results for spirals appreciably, or 10^{-26} gm/cm^3 to affect the results for ellipticals. These densities are very much greater than any that have as yet been suggested.

Another possibility is that the orbits of double galaxies are not stable—that is, they may be moving in hyperbolas. It is unlikely that the pairs are simply galaxies passing one another in near-collisions—their number is over ten times greater than would be expected by chance—but it is possible that they are flying apart because of some unspecified explosion, such as the explosions postulated in explaining the extragalactic radio sources. As the Armenian astronomer Ambartzumian has shown, there is evidence that clusters of galaxies are expanding, possibly from similar causes, and that all groups of galaxies may be fragments of larger, unstable bodies. Although the mechanism that could produce such fission in bodies of more than 10^{12} times the sun's mass, as postulated by Ambartzumian, is difficult to imagine, it may be possible to distinguish statistically between circular orbits and radial motion in the observed pairs (*1*).

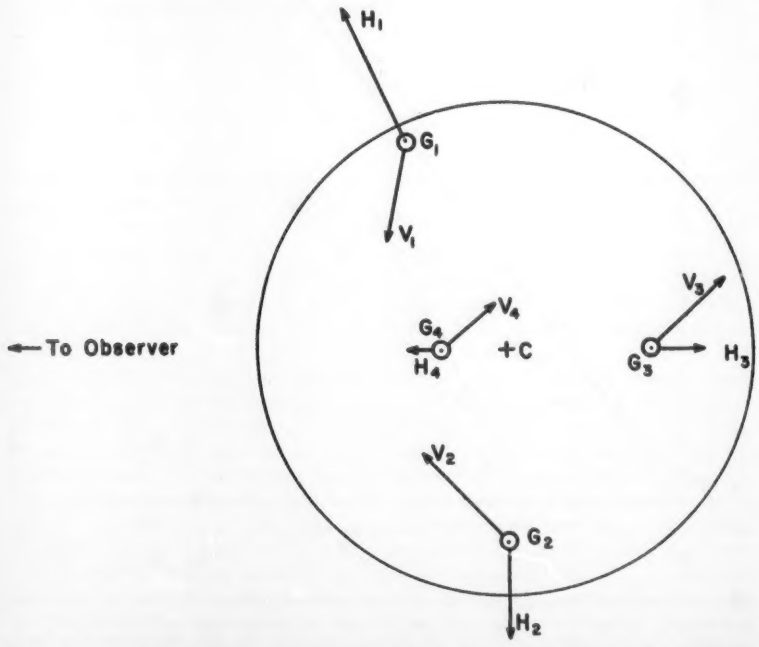

Fig. 2. Centrifugal expansion of a cluster of galaxies. The large circle represents a cluster with center at C and member galaxies G_1, G_2, and so on. The vectors **V** represent random velocity components, and the vectors **H** represent the centrifugal component proportional to the distance from C, as in Hubble's law for field galaxies. The observer, it is presumed, views the cluster from the left.

Expansion of Clusters of Galaxies

As mentioned above, the question as to whether the clusters of galaxies are stable dynamical systems or systems that are flying apart is important in a number of problems of cosmology. Thus far, the arguments in favor of expansion have been based on the fact that, with reference to some clusters or groups of galaxies at least, the observed radial velocities of presumed members of clusters show a consider-

able variation. If this variation is interpreted as the effect of internal motions of galaxies within the cluster, then these motions appear to be too rapid for the gravitational forces to keep the cluster together, unless the masses of galaxies (and thus, the gravitational attraction) are very much larger than is indicated by other studies, such as my study of mass, discussed above. In other words, the problem of stability of clusters has so far been studied on the basis of the relation between the masses of the presumed members of clusters and their velocities.

A paper by J. Lovasich, N. U. Mayall, and E. L. Scott of the University of California throws new light on the problem. The authors combine new observations of radial velocities and apparent magnitudes of galaxies belonging to 20 clusters with a theoretical analysis by Neyman and Scott published in 1953. In their preoccupation with the chance mechanism underlying the hypothetical expansion of the universe, Neyman and Scott posed the question of whether or not, in addition to receding from each other according to Hubble's law, the clusters of galaxies also *expand* in accordance with the same law. In addition to random velocities within a cluster, each member would then have a velocity component in the direction away from the cluster center and proportional to its distance from the center. A statistical test based on apparent brightnesses, radial velocities, and angular distances of presumed cluster members from the cluster center was deduced and applied to the then-existing data for 12 galaxies in the Coma cluster. The result was negative: there was no evidence of a centrifugal component of the velocity among cluster members.

Notably through the persistent work of N. U. Mayall, there are now available measured radial velocities and apparent brightnesses (magnitudes) for 50 galaxies of the Coma cluster. Lovasich, Mayall, and Scott used these data, together with others relating to 19 other clusters and smaller groups of galaxies. In several cases, including the Coma cluster, the test indicates that the observed relation between the apparent brightnesses, positions, and radial velocities could hardly be ascribed to purely random causes—that is, these clusters and groups are probably expanding. In other cases, particularly for clusters and groups with only a few measured velocities and magnitudes, expansion cannot be established with reasonable confidence. However, an over-all test of centrifugal expansion in *all* of the 20 clusters and groups indicates that it is more likely than not that such expansion is taking place.

In Fig. 2 the circle symbolizes a cluster of galaxies with members G_1, G_2, . . . G_i . . . . The vectors $\mathbf{H}_i$ represent the hypothetical Hubble velocity component proportional to distance from the cluster center, and the vectors $\mathbf{V}_i$ symbolize the random velocity components. If not equal to zero, the centrifugal velocity component will create a positive correlation between the radial velocities and the apparent brightnesses of the galaxies.

Underlying the Neyman-Scott test, or any other test of the expansion or

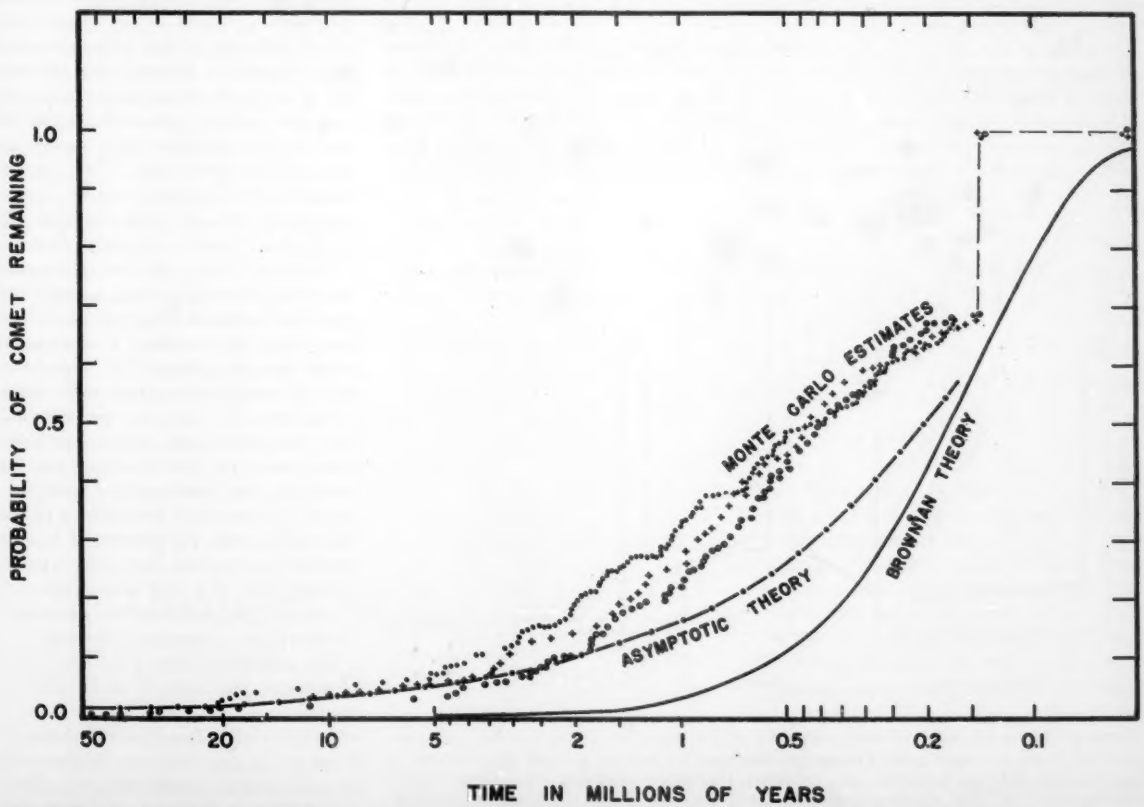

Fig. 3. Time variation of the probability of a comet's remaining in the solar system. Results of three groups of Monte Carlo trials are shown by the dots, circles, and crosses. The asymptotic theory of Hammersley fits at the left end of the curve, and his Brownian motion model fits roughly for short time intervals.

stability of clusters, there is the question, emphasized by the three authors, of whether the galaxies presumed to be cluster members really belong to the cluster concerned. Lovasich, Mayall, and Scott presented their data in plots of radial velocity versus brightness, which showed a few "outliers" for each cluster—probably foreground galaxies superimposed by chance on the photographs. However, even after the elimination of these outliers, the over-all test continues to support the hypothesis of centrifugal expansion, though at a lower confidence level.

In discussion at the symposium it was pointed out that the variation in apparent brightness of galaxies within a cluster ascribable to the variation in distance from the observer in clear space (the inverse-square law) is very small as compared to the errors of measurement and to the inherent variability of the luminosity or candle power of a galaxy. Thus, if there is a sizable correlation between magnitude and radial velocity, it may indicate not only centrifugal expansion but also some other factor causing galaxy G_3 to appear fainter than G_4 (Fig. 2). This factor may be the existence within the boundaries of the cluster of absorbing matter. The possibility of such absorbing clouds within clusters had been suggested earlier, particularly by Zwicky.

Birth and Death of Comets

Comets, the mavericks of the solar system that appear at a rate of two or three per year, were first thought to include real intruders from outer space. Some 50 years ago accurate observations and careful analysis showed that, although they come from great distances, rush around the sun at high speed, and are flung back roughly whence they came, their initial motion is no more than that expected for bodies falling from a large distance under the sun's gravitational attraction; that is, their initial orbits are parabolas or ellipses, not hyperbolas.

However, comets that happen to pass close to a planet are deflected from their parabolic motion by the added gravitational pull of the planet and may, according to chance, end up in smaller, elliptical orbits or in larger orbits, including hyperbolic orbits that carry them away from the sun, never to return. This loss, together with another, directly observed, fate of comets—break-up or evaporation close to the sun—raises the question of how there are any comets left after billions of years' exposure to such hazards.

In a series of highly mathematical papers R. A. Lyttleton of Cambridge, J. M. Hammersley and David G. Kendall of Oxford, and R. H. Kerr of Ferranti, Ltd., all from England, made great progress in answering this question by investigating the average fate of a comet in its perilous journey among the planets, and by analyzing the meager evidence concerning the formation of comets.

On the latter subject, Lyttleton had proposed several years ago that comets are formed from interstellar dust through which the sun is moving. Following the motion of individual dust particles as the sun sweeps by, he found that there would be a concentration of dust in a region beyond the sun up to a few thousand times the earth's distance from the sun. If this material coagulated and fell toward the sun it would provide an almost continuous supply of new comets in the form of swarms of dust particles which would behave just as comets do (growing a tail as the sun "bakes out" gas from the dust and blows it away by radiation pressure).

One feature that can be observed is the direction from which the new comets fall; hence, Lyttleton and Tyror plotted on a sphere the directions from which 448 long-period, long-orbit comets appeared to come during the past 50 or 60 years. Many of these comets may have already been deflected by the planets on previous trips round the sun, but the proportion of new comets should result in a slight statistical preference for the direction opposite that of the sun's motion through space.

Lyttleton's spherical plot gives only marginal evidence of such a preference but is of considerable interest in any case since it reveals, by clusters of points on the sphere, that large numbers of comets come from some directions, and, by gaps on the spherical plot, that none come from other directions. Some of these features may be explained by observational selection, others by statistical preferences in the interaction with the planets. If it were possible to include only new comets making their first trip around the sun, Lyttleton's plot would indicate accurately where the comets were formed.

An alternative theory of the origin of comets, put forth by J. H. Oort of Holland, is that they come from a spherical shell a few hundred thousand times as far from the sun as the earth is, and moving with the sun. Oort postulates that passing stars every now and then give slight impulses to the cometary material in this shell so that

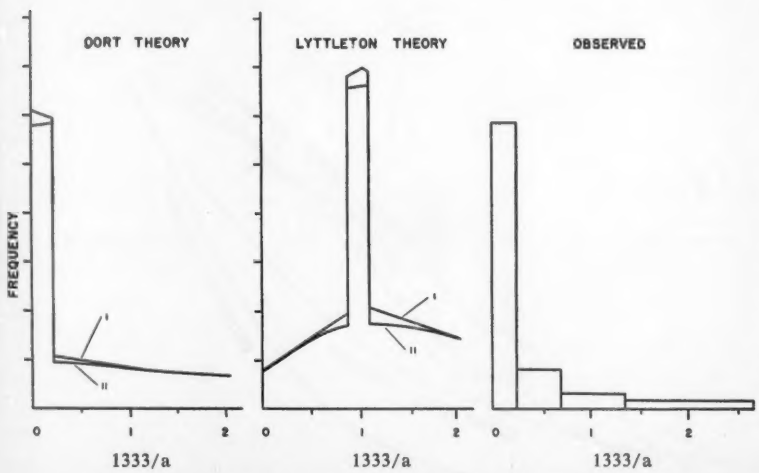

Fig. 4. Energy spectra of observable comets in population equilibrium. Orbital energy increases to the left; a is the semimajor axis of a comet's orbit, one-half the maximum distance from the sun. (Left) Theoretical spectrum for comets formed continuously at near-infinite distance from the sun. (Middle) Theoretical spectrum for comets formed continuously at 2700 astronomical units from the sun. (Right) Estimated spectrum, based on observations of 23 comets. The theoretical curves are for disintegration probability of 0.04 per passage, and the difference between curves I and II reflects different assumptions about the distribution of perturbations.

some of the small condensations fall in toward the sun (with just a little sideways motion so that they miss hitting the sun). Lyttleton's spherical plot, restricted to new comets, would reveal from which part of Oort's spherical shell comets have come, but there is no way of assessing precisely how a passing star would deflect the material. A more decisive test comes from the statistical treatment of comets' energies and their random changes due to the gravitational jerks or perturbations caused by planets the comets happen to pass. Because the space about the sun is so nearly empty, comets move with frictionless ease, and their energy (kinetic plus potential) would be determined only by the distance they fall toward the sun, were it not for planetary perturbations. Accurate observations of some 20 comets show that the difference between energy of approach and energy of recession from the sun is a small random quantity with a mean of zero (the increases balance the decreases) and a certain "spread" or mean deviation that agrees with theoretical predictions by Kerr.

Hammersley attacked the problem of how long a comet can remain within the solar system when its orbit is perturbed in this random manner on each approach to the sun. If a comet gets several large, positive-energy perturbations, its energy exceeds that required for escape and it never returns. One method of studying this problem is by many trials, the so-called "Monte Carlo" method. Hammersley also developed two approximate analytical solutions, one based on the analysis of Brownian motion that holds for a small number of passages and low starting energy and one that is expected to hold for a very large number of passages. The Monte Carlo runs, over 1.5 million on the Ferranti machine at Harwell and 1700 on the IBM 704 at the California Institute of Technology, confirm the analytical results over long time intervals, as shown in Fig. 3. Hammersley found that, depending slightly on the starting energy (that is, the distance from which new comets start falling toward the sun), the probability of a comet's remaining in the solar system decreases to 1 percent in about 200 million years. If a 4 percent chance of break-up on each passage is assumed, the decrease is a good deal more rapid, all comets being lost or disintegrating in a few million years.

The conclusion drawn by Lyttleton and Hammersley is that comets must be replaced at a fairly continuous rate. A possible alternative conclusion is that the number of comets was 1500 to 15,000 times greater when the solar system was formed, some 5 billion years ago, than it is today. (It is estimated that there are now 40 million comets.)

Adopting the former conclusion, Kendall examines the question: What energies should comets have at present if they are being formed as rapidly as they disintegrate or are lost? Using analysis similar to Hammersley's, Kendall derives the spectrum of energies which would result if new comets were all being dropped into the sun from a distance about 3000 times the earth's distance from the sun. This spectrum is modified for the effects of observational selection, account being taken of the fact that we see comets on long orbits less frequently than comets on short orbits, and the results are compared with the energies of 23 comet orbits measured accurately in the interval 1850 to 1936.

Figure 4 (middle and right) shows that confirmation is lacking; Lyttleton's theory predicts a maximum of comet energies corresponding to the 3000-unit starting distance, while the observations show a peak at energies corresponding to much larger distances. In a second trial Kendall assumed that the new comets fall from almost infinite distance, as in Oort's theory, and this theoretical spectrum of energies agrees quite well with the observations, as shown in the left and right curves of Fig. 4.

The tentative conclusion is that new comets are being formed at very great distances—10,000 to 100,000 times the earth's distance from the sun—and added to the diminishing number that we see circulating among the planets. The data cannot prove that the total number remains constant over long periods of time; in fact, it appears that *more* new comets have been added in recent times than were needed to keep the observed population constant. If Oort's theory is correct, the sun may at present be closer than usual to stars that disturb its shell of cometary material—a subject for further statistical investigation (*2*).

References and Notes

1. This is the subject of an unfinished investigation by Neyman, Page, and Scott.
2. Proceedings of the 4th Berkeley Symposium, including a full account of each of the investigations outlined in this article, will be published by the University of California Press in 1961.
3. B. Y. Mills, O. B. Slee, E. R. Hill, *Australian J. Phys.* **11**, 360 (1958).
4. C. O. Edge *et al.*, *Mem. Roy. Astron. Soc.* **68**, 37 (1959).

Thurlow Christian Nelson, Marine Biologist

Thurlow Christian Nelson, marine biologist, was drowned on 12 September, 1960, off a storm-swept shore near his summer cottage at Green Creek, Cape May, N.J., while trying to secure his rowboat against hurricane Donna. He would have been 70 years of age on 22 September.

He was born in Highland Park, N.J., in 1890 and attended Rutgers elementary and preparatory schools in New Brunswick, just across the Raritan River from his home. He graduated from Rutgers University in 1913 with a B.S. degree in biology, and from the University of Wisconsin in 1917 with a doctorate in zoology and physiological chemistry. During World War I he served as a first lieutenant in the Army Sanitary Corps.

He was invited to join the Rutgers teaching staff in 1919 as assistant professor of zoology, becoming associate professor in 1922 and professor in

1926. From 1925 to the time of his retirement in 1956 he was chairman of the department of zoology, and he was biologist in charge of shellfish investigation at the New Jersey Agricultural Experiment Station from 1916 to 1950. In addition he served as chairman of the New Jersey State Water Policy and Supply Council from 1945 until his death. After his retirement he was named Julius Nelson Professor of Zoology in the Rutgers Graduate School, a chair founded in memory of his father, who began oyster research in New Jersey for Rutgers in 1888.

In 1934 Thurlow Nelson was honored as Rutgers' Distinguished Scholar and Gifted Teacher. Five years later the university awarded him an honorary degree of doctor of science and in 1958, the Rutgers Alumni Federation Award.

In *American Men of Science* Nelson's specialties are listed as biology of the oyster, estuarine ecology, marine biology, and limnology. His research in these areas resulted in more than 125 papers on the anatomy, physiology, and ecology of the oyster and associated organisms; on parasitology; and on water supply.

Thurlow C. Nelson

Nelson held membership in many scientific societies. He served as president of the American Society of Limnology and Oceanography in 1953, as president of the National Shellfisheries Association from 1931 to 1933, as vice president of the American Society of Zoologists in 1948, and as vice president of the American Microscopical Society in 1941.

Viewed in perspective, this long list of accomplishments is an impressive one. Equally impressive were Nelson's contributions as a teacher over a span of some 45 years. His keen mind, warm personality, sincere and forceful speech, genuine interest in people, and deep enthusiasm for biology attracted many students to him. As an active church leader with deep religious convictions, Nelson often wrote and spoke on the common ground of religion and science. He died in the midst of the elements which provided so much of the stimulus for his productive life.

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Department of Zoology and
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University of North Carolina

Science in the News

Castro and Kennedy

From Havana: During the past weeks the two men in this hemisphere who clearly command the attention of the world moved to consolidate their positions in preparation for the political struggles they will have to face in the year ahead. Kennedy's artful selection of his cabinet and Castro's preparation for the liquidation of remaining organized opposition to his regime offer an interesting contrast between the politics of democracy and the politics of totalitarianism. The precision with which both followed the precendents of earlier men in their respective positions suggests that there is indeed a science of politics. Kennedy moved to pacify his opponents, and even to win some of them over; Castro, since the politics of extremism, to which he seems wholly committed, allows little chance of pacifying his opposition and no chance of winning it over, moved to lay a basis for future claims that his actions are forced upon him by his utterly unreasonable foes.

Castro's situation requires that he try to destroy his present opposition now, while he still commands the strong support of the mass of the Cuban people. He began with nearly unanimous support, and as he has moved further and further left, he has so far been able to deal effectively with each wave of defections, one by one; a necessary policy since if he allowed his enemies to accumulate they would eventually overwhelm him.

Castro's position thus far has been unassailable, partly because of the genuine reforms he has brought to Cuba, partly because the austerity and restrictions that forced industrialization will require are still in the future, partly because of the force of his personality. But when the genuine reforms come to be taken for granted, when disenchantment sets in as the people realize that things are not going to be quite the way they thought Fidel was saying they would be, the regime will necessarily begin to lose some part of its mass support. Against that day, Castro moved last week to throttle the two remaining centers of organized opposition to whom defections among the masses could rally.

Last week Castro moved to destroy the last important anticommunist figure in organized labor in Cuba, Fraginals, the president of the electrical workers' union, who had recently led his men in the first open anticommunist demonstration in Cuba in a year. The union had held a demonstration in front of the presidential palace, chanting "Cuba, si! Russia, no!"

Soon afterward the newspaper began to be full of stories of the "traitors" in the electrical workers' union, of sabotage and bombings by certain members

of the union, of how the electrical workers would rid themselves of these traitors, and finally of a mass meeting of the electrical workers sponsored by the communist-dominated central trade union council which would expel the traitors. Castro himself delivered the principal address at the mass meeting. "Do you know the fundamental aim for which the working class must fight in a modern state? For the conquest of political power!" "The government of Cuba" he said, "is now in the hands of the oppressed and the exploited. . . . The revolutionary government . . . and the Cuban people are now the same thing." The only implication to be drawn from Castro's speech was that he intended to stamp out any dissent from what the revolution was doing and was going to do in the name of the "oppressed and exploited," and, since only one point of view was to be permitted, and the communist point of view was permitted, that Castro, although not a party member himself, must feel that his point of view is identical with that of the communists.

There is a saying in Cuba that "Fidel is the soul of the Revolution, but Che is the brains." Two weeks ago Che Gueverra, the head of the state bank who is in charge of over-all direction of the Cuban economy, told a Moscow audience that the recent official Communist declaration citing Cuba as an example of what the other underdeveloped nations of the world ought to do was correct, "although we are not yet united in a single political party." Once again the implication was clear: if the aim is to have a single party, and if the Communist party in Cuba is being permitted to strengthen its position, then it follows that the leaders of the revolution must expect an eventual consolidation of the Communist party with the official revolutionary organization, the 26th of July Movement.

Beyond ridding himself of his present open opposition, what Castro appears to be doing is to lay a basis now, when his supporters are in no mood to question anything he says, for a purge of the "soft" members of his own government. For although the revolution is frankly socialist, what appears to be a very substantial minority, perhaps a majority, of his more sophisticated supporters are likely to find themselves disappointed in the year ahead. They expect two things from the government which they are not likely to get: a reconciliation with the United States and a willingness to slow the pace of the revolution rather than to have to resort to severe repression to bring about industrialization. The most interesting parts of Castro's speech at the meeting which expelled the leaders of the electrical workers union were entirely unnecessary so far as the general public is considered.

A small survey of people in the streets during the days after the meeting showed that the public had readily accepted the newspaper stories that the union leaders had been spies or traitors or saboteurs. But by making it quite clear to a reasonably alert listener to his speech both that he saw no difference in his point of view and that of the communists and that, in the name of the revolution, no dissent from this point of view would be permitted, Castro left little basis for those who so readily accepted the Fraginals incident to complain when the time came to apply the same principles to members of his own 26th of July Movement.

In fact, very few of these people are alert. Up to this point an excellent case can be made that revolution has done so much good for Cuba, without having to resort to a real reign of terror, that whatever criticisms can be made are unimportant in comparison with what has been accomplished. Castro's noncommunist supporters are in no mood to be critical, to interpolate what Castro is saying now to try to understand what he means to do in the future.

If the mood to be critical comes upon them they feel that there is really nothing at all they can do. For to be openly critical would be to be a counter-revolutionary, and they do not as yet feel strongly enough about their criticisms to become counter-revolutionaries. They try, quite successfully, not to think about it. Meanwhile, Castro moves about destroying those who are already counter-revolutionary and who might be their allies when their own time comes.

Two days after the Fraginals incident Castro spoke about the Catholic Church, the only scource of public opposition left in Cuba, and about the judiciary, the only part of the Cuban government which is not entirely in his hands. He said that the entire power of the state must be at the service of the people, and that something would have to be done about judges who obstruct the course of the revolution. He said the revolution would not interfere with the church if the church did not interfere with politics. "To be anticommunist is counter-revolutionary," he said. "To be anti-Catholic or anti-Protestant, or to be anything which tends to divide the people is counter-revolutionary." Thus the revolution, he is saying, will bother no one so long as he does not bother the government. "There is only one road for the revolution," he said, "and that is to move forward." After the people have had time to digest a purge of the judiciary and the arrest, which is surely not far off, of those church leaders who insist on criticizing the government from the pulpit, the only place from which the government can be criticized in Castro's Cuba, then Castro will be free to move forward.

Kennedy's Cabinet

While this was going on, Kennedy, who also talks of moving forward, prepared for *his* advance. His method has been to choose a cabinet which so far as possible disarms his opposition without in any important way compromising his purpose. During the campaign he was accused of wanting to spend so much that he would ruin the value of the dollar. So he has chosen an emminently respectable Republican banker as Secretary of the Treasury, but a Republican banker who has in no way been identified with Eisenhower's fiscal policies, and whose broader understanding of what is going on in the world is indicated by his distinguished service as Under-Secretary of State. In the State Department Chester Bowles was given only the number two spot, almost certainly because it would add to the difficulty of pushing a controversial program through Congress to have it presented by a man who, as the principal author of the Los Angeles platform, is so closely identified with a controversial domestic program.

So also the third great job, Defense, went to a man who would tend to disarm the opposition, in this case the opposition to a real reorganization of the Defense Department which has been talked about by everyone for years and about which nobody has been able to do anything. Who better to convince Congress of the way a large organization ought to be run than the vigorous, thoroughly competent president of a giant corporation, assuming, as there is reason to do, that McNamara is nearly as different from Charles Wilson as Kennedy is from Eisenhower.

Kennedy's gentle maneuverings are rather tame compared to Castro's grand sweep—H.M.

News Notes

Snow Pleads for More Scientists in Government: Warns of Secret Politics, Citing Wartime Incident

Scientists are needed in the top levels of government to keep "our sense of the future" in focus, says C. P. Snow, British physicist and novelist. Snow, who has explored the lives of modern scientists in a series of novels, *Strangers and Brothers*, gave the last of three Godkin Lectures on "Science and Government" at Harvard University early this month. He pointed out that, by contrast with the Communist nations, which are "future directed societies," the nations of Western Europe and the United States are becoming "existential societies." He said, in part:

"We seem to be flexible, but we haven't any model of the future before us. In the significant sense, we can't change. And to change is what we have to do.

"*That* is why I want scientists active in all the levels of government. By 'scientists' here I mean people trained in the natural sciences, not only engineers, though I want them too. I make a special requirement for the scientist proper, because, partly by training, partly by self-selection, they include a number of speculative and socially imaginative minds. . . . Engineers—more uniform in attitude than one would expect a professional class to be—tend to be technically bold and advanced but at the same time to accept totally any society into which they may have happened to be born. The scientists proper are nothing like so homogeneous in attitude, and some of them will provide a quality which it seems to me we need above everything else.

Soviet Scientists in Government

"I don't merely mean here that, if we had scientists of any kind diffused through government, the number of people helping to influence secret choices is bound to increase. That is true. In my view, it would be a real gain. It is a clear advantage to the Soviet Union that they have, right at the top of the political and administrative trees, a fairly high proportion of men with scientific or technical training. The proportion of these men in the top executive organs, or among high-ranking diplomats, seems to be somewhere between 35 and 45 percent, which is far higher than in the U.S. or England. In the fields where they have made better technical choices than either of us—and there are plenty—this collective influence has no doubt been a help. But, though that is a real gain, it is secondary to what I have most in mind.

Urgent Need for Foresight

"I believe scientists have something to give which our kind of existential society is desperately short of—so short of that it fails to recognize of what it is starved. That is foresight.

"I am not saying, of course, that all scientists have foresight and no one else has. Foresight is a fairly rare quality . . . it isn't quite knowledge. It is much more an expectation of knowledge to come. It is something that a scientist, if he has this kind of sensitivity . . . , picks up during his scientific experience. . . .

"Any scientist realizes that his subject is moving in time—that he knows incomparably more today than better, cleverer, and deeper men did 20 years ago. He knows that his pupils, in 20 years, will know incomparably more than he does. Scientists have it within them to know what a future directed society feels like, for science itself, in its human aspect, is just that. That is my deepest reason for wanting scientists in government. . . .

"We [in the West] are immensely competent; we know our own pattern of operations like the palm of our hands. It is not enough. That is why I want some scientists mixed up in our affairs. It would be bitter if, when this storm of history is over, the best epitaph that anyone could write of us was only [this]: 'The wisest men, who had not the gift of foresight.' "

The Danger of Secret Decisions

To illustrate his thesis of the need for more scientists in government, Snow described, in the second of his Godkin Lectures, a heretofore undisclosed incident that took place in England during World War II as an example of the risk involved in the "secret choices," made by a mere handful of men, that today critically affect the future of the advanced countries, now so dependent on science and technology. He told the story of conflict between Sir Henry Tizard, the man who pushed the development of radar before World War II, and Lord Cherwell, who was Winston Churchill's scientific adviser.

In 1942, Lord Cherwell urged strategic bombing. Tizard and P. M. S. Blackett, another physicist, found, on scientific grounds, that Lord Cherwell's estimates of the effectiveness of strategic bombing were five or six times too high. This indicated that a different strategy, both for production and for use of troops, was required. But Lord Cherwell's policy, backed by the Air Ministry, was put into effect, and Tizard was pushed out of the scientific councils of Great Britain.

After the war's end, the strategic bombing survey found that Cherwell's estimates had been ten times too high. Snow commented:

"It was not easy for [Tizard] to be called a defeatist. It was even less easy to be shut out of scientific deliberations, or to be invited to them on condition that he did not volunteer an opinion unless asked. It is astonishing in retrospect that he should have been offered such humiliations. I do not think that there has been a camparable example in England in this century. . . .

"He believed, both in his dignified exile in Oxford and to the end of his life, that if he had been granted a fair share of the scientific direction between 1940 and 1943, the war might have ended a bit earlier and with less cost. As one goes over the evidence, it is hard not to agree with him."

A new book by Snow entitled *Science and Government*, from which the Godkin Lectures are drawn, will be published next spring by Harvard University Press. It will be a Book-of-the-Month Club selection.

Seaton Sets Aside Alaskan Wildlife Areas

Secretary of the Interior Fred A. Seaton announced on 8 December that he had issued orders reserving some 11 million acres of federal lands in Alaska for wildlife. Some 8,900,000 acres have been set aside for the Arctic Wildlife Range, 1,400,000 acres for the Kuskokwim National Wildlife Range, and 415,000 acres for the Izembek National Wildlife Range. Conservationists have pressed for such action for years.

The Arctic Wildlife Range is to be located in northeastern Alaska in an area not presently inhabited. Canada is expected to establish a comparable, and adjacent, area. The ranges are needed to protect Arctic caribou, grizzly bears, Dall sheep, moose, and marine animals,

whose numbers are dwindling, and to provide nesting grounds for migratory waterfowl, as well as to preserve a unique portion of the Arctic for scientific study.

The Kuskokwim and Izembek National Wildlife Ranges are located on lands in the public domain. The Kuskokwim and Izembek regions are important, respectively, as nesting and congregating areas for waterfowl.

Enrollment Leaps, But More Is Spent on Gambling than Education

Fall enrollment of full- and part-time students reported by the nation's colleges and universities at the beginning of the 1960–1961 scholastic year reached the all-time high of 3,610,007, the U.S. Office of Education reports. The 1960 fall enrollment exceeds the 3,402,297 enrollment of 1959 by 6.1 percent. This is the ninth consecutive year that a rise has been reported. The number of students enrolled in college for the first time rose to 929,823, a jump of 12.4 percent over 1959.

The 1960 enrollees included 2,270,640 men and 1,339,367 women as compared with 2,173,797 men and 1,228,500 women in the fall of 1959. The percentage increase for men was 4.5; for women it was 9.0.

Yet, despite these figures, the Council for Financial Aid to Education reports that Americans spend $20 billion a year for legalized gambling, while only $4.5 billion goes for higher education. The council also reports that experts estimate that by 1970 college applications will have doubled. And the tuition usually paid by a college student covers less than half the actual cost of his education. The balance must somehow be made up by the institution.

Latin-American Medical Research Agreement Announced

An agreement to aid in the administration of medical research activities in the countries of the Americas has been announced jointly by the Pan American Health Organization and the United States Public Health Service. The agreement, issued as a "statement of arrangements" between the two organizations, focused on three primary points: (i) staff collaboration between the two organizations, (ii) development of PAHO research activities, and (iii) definition of forms of Public Health Service aid that might be applied to PAHO research activities.

The Pan American Health Organization will provide moderate financial support to certain research projects and programs, conduct research by its own staff, provide coordination for research projects involving more than one country, and aid in the development of scientists, scientific communication, and other medical research activities.

The U.S. Public Health Service, under the agreement, will provide technical advice on research design. In addition, PHS will consider research-grant proposals from investigators who may wish to participate in research programs coordinated by PAHO and applications for support to PAHO for research conducted or coordinated by the PAHO staff.

Continuing responsibility within the Public Health Service for development of program plans and specific details will be delegated by the Surgeon General to the Office of International Research Activities at the National Institutes of Health.

News Briefs

Geneva talks adjourn. The international nuclear test-ban conference in Geneva is in recess until after President-elect John F. Kennedy takes office. The United States proposed that the talks adjourn until 7 February, and Britain and the Soviet Union readily agreed. The final session, on 8 December, was the 273rd of the 2-year-old conference.

* * *

Californium. The first pure chemical compounds of the man-made element californium have been produced at the University of California, Berkeley, by Burris B. Cunningham and James C. Wallman of the Lawrence Radiation Laboratory. Californium is element 98 in the periodic table. The compounds—californium trichloride, californium oxychloride, and californium oxide—were prepared by treatment of californium and hydrochloride acid and steam at high temperatures.

* * *

Radiation protection. The Federal Radiation Council is considering the problem of providing radiation protection guides for selected radionuclides to be used in the control of human exposure from environmental sources. The council invites public comment on the basic issues involved in this problem. To insure effective consideration, comments should be transmitted by 14 February 1961. For an outline of the problem, copies of *Radiation Protection Guidance for Federal Agencies*, and of the related Staff Report No. 1, may be obtained from: Chairman, Federal Radiation Council, Executive Office Building, Washington 25, D.C.

* * *

NSF awards. The National Science Foundation has announced 376 science faculty and senior postdoctoral fellowship awards for 1961. Some 285 college and university faculty fellowship winners were selected from 754 applicants from all parts of the United States and its territories; 91 recipients of senior postdoctoral fellowships were selected from 275 applicants.

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Report on Soviet abstracting. The National Federation of Science Abstracting and Indexing Services (301 E. Capitol St., Washington 3, D.C.) has released *Some Counterparts in Perspective*, a detailed report on visits in 1959 to the Soviet All-Union Institute of Scientific and Technical Information, the Polish Central Institute for Documentation in Science and Technology, the Excerpta Medica Foundation, and the Danish Technical Information Service. The volume was prepared by D. B. Baker, G. M. Conrad, J. C. Green, M. Hoseh, and R. A. Jensen.

* * *

UNESCO survey. UNESCO's South Asia Science Cooperation Office is conducting a survey in South Asia of nongovernmental organizations for the promotion of science. The office has requested associations and societies in the fields of the natural sciences to complete a questionnaire. Scientific organizations which may have been overlooked are requested to communicate as soon as possible with the UNESCO Office (21 Curzon Road, New Delhi 1, India).

* * *

Biology style manual. The American Institute of Biological Sciences (2000 P St., NW, Washington 6, D.C.) has announced that the new *Style Manual for Biological Journals* is available at $3 a copy. The manual is designed for biologists preparing manuscripts for publication in scientific journals. Style is interpreted broadly to include forms of expression in scholarly writing and

general technical requirements of journals, such as form of typed manuscripts, standard abbreviations, and form of reference citations.

The Conference of Biological Editors' Committee on Form and Style, under the chairmanship of J. R. Porter, worked for 3 years in preparing the manual, which was read in draft form by approximately 100 editors and publishers. The editorial boards of 76 biological journals have adopted the manual in whole or in part.

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Massachusetts Hospital anniversary. The Massachusetts General Hospital will celebrate its 150th anniversary with a 3-day convocation 31 January–2 February. An extensive program of scientific symposia has been planned. For information, write to Joseph E. Garland, Massachusetts General Hospital, 275 Charles St., Boston 14, Mass.

* * *

Research association formed. A new organization, the British Industrial Biological Research Association, has been formed in England to study the possible effects upon health of substances which may be ingested in food, drink, and cosmetics. Supported by a number of leading companies, it is welcomed by interested government departments and will receive a grant from the Department of Scientific and Industrial Research. The association's main object is the establishment of a biological reresearch station. Although some of the larger companies in the food and chemical industries have their own research facilities, until now there has been no national industrial organization in England with responsibility for work of this kind.

Grants, Fellowships, and Awards

Cerebral palsy. The United Cerebral Palsy Research and Education Foundation, Inc. (321 W. 44 St., New York 36, N.Y.) has announced that the following changes have been made in the deadlines for submission of applications for research and training grant support by the foundation. For funds made available on 1 July, the application deadline is *15 January*; for funds available on 1 January, the deadline is *15 July*. Applications received after these deadlines will automatically be held over for consideration during the subsequent fund-granting period.

However, the foundation has allocated a certain portion of its funds as fluid capital, which enables it to consider, between the usual meetings of its scientific advisers, special projects that may require support on an emergency basis. Applications for the support of projects of this nature will be considered for prompt action if, in the opinion of the foundation, the circumstances warrant such consideration.

Foreign. The twelfth edition of the United Nations' *Study Abroad* is now in print. It contains information on some 100,000 individual opportunities for international study and travel in 1961 and 1962. These scholarships and fellowships are offered in a wide range of fields and at all educational levels; they vary in duration from about 2 weeks to 8 years.

The new edition may be procured from the UNESCO Publications Center and from the Columbia University Press. Approximate price, $3.

General, for women. Sigma Delta Epsilon, Graduate Women's Scientific Fraternity, has announced a small grant that is available to any woman who (i) holds a degree from a recognized institution of higher learning and (ii) has demonstrated outstanding ability in research in one of the mathematical, physical, or biological sciences. The award may be applied either directly to a research project or to course work, provided the latter is relevant to the proposed research program.

Appointments will be made irrespective of race, nationality, creed, or marital status. Preference will be shown to applicants 35 years of age or older, but this limitation is not absolute.

Application blanks may be obtained from Dr. Ruth V. Dippell, Zoology Department, Jordan Hall 218, Indiana University, Bloomington, Ind. The application deadline is *1 February*.

Miniaturization. Miniature Precision Bearings, Inc., invites entries for its Miniaturization Award competition, conceived to give recognition to the outstanding miniaturization achievements of the year and to increase public awareness of significant advances in the field of miniaturization. For information and entry forms, write to the Miniaturization Award Committee, Box 604, Keene, N.H. The deadline for the 1960 competition is *10 January 1961*.

Rumania and Poland. The Institute of International Education has announced that fellowships for graduate study in Rumanian and Polish universities are being offered to American students for the academic year 1961–62. The awards are open to both men and women, in any field of study. Candidates must be at least 21 years old, must be U.S. citizens, and must have the necessary foreign language. While married persons may apply, no funds will be available for the support of dependents. Applicants for the Rumanian scholarships should have at least a year of graduate training; a bachelor's degree is required of candidates for the Polish awards.

Application forms, which must be returned by *15 January*, may be obtained from the Institute of International Education, 1 E. 67 St., New York 21, N.Y., or from one of the institute's regional offices.

Social sciences. The Division of Social Sciences of the National Science Foundation has announced that the next closing date for receipt of basic research proposals in the social sciences is *1 February 1961*. Applicants will be advised approximately 4 months later of the disposition of proposals received prior to that date. Proposals received after the February closing date will be reviewed after the spring closing date of 1 May. Inquiries should be addressed to the National Science Foundation, Washington 25, D.C.

Scientists in the News

George Feher, on leave from Bell Telephone Laboratories as professor of physics in the School of Science and Engineering at the University of California, La Jolla, received the American Physical Society Prize of $2500, sponsored by the Hughes Aircraft Company, during the society's annual Thanksgiving meeting. Feher was honored "for originating and developing the electron-nuclear double resonance technique and for applying it in solid state and nuclear research," work he conducted while at the Bell laboratories.

Charles J. Fish, professor of marine biology at the University of Rhode Island, has resigned from his administration duties as director of the Narragansett Marine Laboratory, North Kingston. He asked to be relieved of these duties because of the growing program of basic research supported by the Navy, for which he is primarily responsible. In addition to heading this work he will continue as a faculty member. Fish has directed the Marine Laboratory for 25 years. In October a new laboratory building was dedicated in his honor.

Ralph P. Stein, formerly director and technical adviser of the AEC-supported Heat Transfer Research Facility at Columbia University, has accepted a position with the Nuclear Development Corporation of America as advisory engineer and manager of the engineering department, Heat Transfer Section.

The Institute of Physics and The Physical Society of Great Britain, which recently amalgamated, have made three recent awards.

J. B. Adams, the director-general of the European Organization for Nuclear Research, has received the Duddell Medal for his leadership at CERN.

A. W. Merrison of the University of Liverpool has received the Charles Vernon Boys Prize for his distinguished research in experimental physics.

S. E. Forbush of the Carnegie Institution of Washington has received the Charles Chree Medal and Prize for his work on cosmic radiation.

Eugene Sullivan, honorary chairman of the board and a director of Corning Glass Works, has been named the 1961 recipient of the Albert Victor Bleininger Award. This award, presented annually by the Pittsburgh Section of the American Ceramic Society, is one of the highest honors conferred in this country for distinguished achievement in the field of ceramics.

Sullivan, a chemist who is often called the founder of organized research in glass, began his work with Corning in 1908. He is best known for the development of boro-silicate glasses, such as those used in Pyrex cooking and dinner ware and in chemical glassware.

Janwillem van den Berg of the University of Gröningen in the Netherlands has received the 1960 Gould Award of the William and Harriet Gould Foundation for his contributions to laryngeal physiology. The award is presented annually for outstanding laryngeal research. The candidate is selected by an international committee, of which Hanis von Leden of 30 North Michigan Ave., Chicago, Ill., is at present a member.

James F. Haggerty recently joined the staff of the National Cancer Institute as chief, Research Grants Branch. He was formerly associated with the U.S. Atomic Energy Commission, having spent 9 years on the staff of the Division of Biology and Medicine as biochemist, Medical Research Branch.

H. R. Copson, supervisor of the corrosion section of the research laboratory, International Nickel Co., Inc., Bayonne, N.J., has been awarded the Willis Rodney Whitney Award in recognition of his public contributions to the science of corrosion. The National Association of Corrosion Engineers will present the award to Copson during its 17th annual conference, to be held 13–17 March in Buffalo, N.Y.

Lawrence J. Giacoletto, for 4 years manager of the electronics department of the Ford Motor Company Scientific Laboratory, has been appointed professor in Michigan State University's department of electrical engineering.

G. H. Chidester, chief of the U.S. Division of Pulp and Paper, U.S. Forest Products Laboratory, Madison, Wis., has been named recipient of the 1961 TAPPI Medal, which is administered by the Technical Association of the Pulp and Paper Industry. The presentation will take place during the association's annual meeting, to be held 20–23 February 1961, at the Hotel Commodore in New York.

Abraham Bavley, former assistant director of research for the Gillette Safety Razor Company, has been named manager of the Research Division at the Philip Morris Research Center in Richmond, Va.

The Radiological Society of North America recently awarded gold medals to **Thomas B. Bond** of Fort Worth, Tex., and **Warren H. Cole**, professor and head of the department of surgery at the University of Illinois College of Medicine, Chicago. **Howard P. Doub** of Detroit, Mich., was also honored by the society, receiving a silver tray commemorating 20 years as editor of the journal *Radiology*.

I. T. Haig, secretary general of the Fifth World Forestry Congress, has retired from active service and returned to his home in Asheville, N.C. He has been appointed Carnegie visiting professor of forestry at the University of Hawaii for the academic year 1962–63. Prior to his return to the United States from Rome, in 1958, to take up his duties in connection with the Forestry Congress, Haig was chief of the Branch of Research and Technology in the Forestry and Forest Products Division of the Food and Agriculture Organization.

Recent Deaths

Clifford C. Carr, Wantagh, N.Y.; 61; assistant dean of the School of Engineering at Pratt Institute in Brooklyn and a member of the Pratt faculty since 1920; 9 Dec.

Jaques Cattell, New York, N.Y.; 56; editor of *American Men of Science* and *Leaders in Education*, publisher of the *American Naturalist*, and president of the Jaques Cattell Press, Inc.; established the *Directory of American Scholars* in 1942 under the sponsorship of the American Council of Learned Societies; president of the Science Press Printing Company and pioneer in the use of cold-type composition in scientific publishing; coeditor of *Science*, 1944-45; published a number of books in his "humanizing science series"; 18 Dec.

John T. Lonsdale, Austin, Tex.; 64; director of the Bureau of Economic Geology at the University of Texas and professor of geology; 5 Oct.

Walter Noddack, Bamberg, Germany; 67; widely known scientist who, with his wife, discovered the element rhenium; headed the State Research Institute for Geochemistry, served as a professor at the University of Freiburg, then worked for many years at the University of Berlin; 7 Dec.

E. Vernon Potter, Ventura, Calif.; 52; director, Physics and Electronics Division of the U.S. Naval Civil Engineering Laboratory, Port Hueneme, Calif.; his entire career was in government research laboratories—the Bureau of Mines, the Bureau of Ships, and the Bureau of Yards and Docks—where he was recognized as an authority in the fields of underwater accustics and electromagnetic radiation; 6 Oct.

Philip A. Shaffer, St. Louis, Mo.; 79; professor of biochemistry at Washington University and former dean of the School of Medicine; first faculty member to receive the title "distinguished service professor"; member of the National Academy of Sciences; specialized in kidney function and, with Alexis Hartmann, devised what became a standard test to determine the amount of sugar in blood; 4 Dec.

Erratum: In the abstract "Binding of reduced diphosphopyridine nucleotide to yeast alcohol dehydrogenase according to chemical relaxation by temperature jumps," by Georg Czerlinski [*Science* **132**, 1490 (18 Nov. 1960)], the four equations at the bottom of column 1 should have read:

$$YADH + H^+ \rightleftharpoons YADH_2^+$$

$$YAD^- + H^+ \rightleftharpoons YADH$$

Equilibria involving hydrogen bonding

Equilibria involving refolding of enzyme(-parts)

Book Reviews

The New Nigerian Elite. Hugh H. Smythe and Mabel M. Smythe. Stanford University Press, Stanford, Calif., 1960. ix + 195 pp. Illus. $5.

The Smythes' study of the emergent Nigerian elite is timely not only because of Nigerian independence but because there is pressing necessity to understand the leaders of nascent nationalism in Africa generally. In the latter context, this book is recommended as providing knowledge about that sort of elite without which, as in the case of the Congo, independence becomes chaos.

The authors show some conceptual and methodological inelegance in the derivation of their list of the 156 people who constitute the upper-level new elite. The list can, however, be accepted as including a large proportion of the Nigerians in top prestige and power positions in institutions deriving from Western contact. The contribution of the study lies in its analysis of the characteristics of this group, its regional variations, and how it came into being. The new data collected in 1957–58 are set in a historical framework, the early portion of which leans heavily on Sir Alan Burns' *History of Nigeria*, reproducing its anthropological inaccuracies but eliminating its British colonial bias. Discussion of the more recent period traces the development of the elite from their beginnings in the urbanization and westernization of Nigeria.

One of the clearest factors in the formation of the elite is the impact of Western education. All of the top elite speak English, and almost half hold one or more university degrees, largely from foreign institutions. Seventy percent occupy government-connected positions of some sort, which is to be expected in a country whose productive and commercial activities are either small-scale or in foreign hands. Businessmen make up only 12 percent of the elite, and only three trade union officials are found in it. A majority of the top government officials are professional men, and only nine members of the elite are traditional rulers who have assumed roles in the new forms of government. The break with the past is not abrupt, however, for a third of the new elite come from old ruling families.

An insightful part of the study analyzes the style of life of the elite, who have assumed many of the manners of the colonial British, living in Western style houses equipped even to electric fans, maintaining staffs of servants, and belonging to social and sports clubs. Divergences from the British pattern are also revealing. Although the elite work in European clothes, they assume native dress at home, for comfort, and at important social functions, as a mark of national identification. While the physical attributes of the home are Western, the level of housekeeping is low by Western standards, for the wives of the elite do not have the same background as their husbands.

As the elite have assumed the power and manners of the British, they have also inherited the public image of "exploiters." The acquisition of the amenities which are the indentifying marks of the elite is, in the popular mind, evidence of the self-seeking of public officials. The idealistic forces for freedom and national improvement which the elite symbolize are in conflict with their class values.

The new elite with roots in the old are politically more conservative than their self-made colleagues. With the progressive realization of independence, the radicals have, in a sense, lost their cause by winning it. The moderates are in control.

Understanding of the forces which shape the elite runs throughout the Smythes' account of the formal characteristics of the group. It is this which makes social science of what might have only been a social survey. It is social science without jargon. A unique and unbiased work on an important subject, the book is worthy of wide attention.

HORACE M. MINER
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The Kirtland's Warbler. Harold Mayfield. Cranbrook Institute of Science, Bloomfield Hills, Mich., 1960 (order from University Publishers, New York). xv + 242 pp. Illus. $6.

Life Histories of Central American Birds. 2. Families Vireonidae, Sylviidae, Turdidae, Troglodytidae, Paridae, Corvidae, Hirundinidae, and Tyrannidae. Pacific Coast Avifauna No. 34. Alexander F. Skutch. Cooper Ornithological Society, Berkeley, Calif., 1960. 593 pp. Illus.

Bird Portraits in Color. Two hundred ninety-five North American species. Thomas S. Roberts. Revised by Walter J. Breckenridge and others. University of Minnesota Press, Minneapolis, 1960. Illus. $5.95.

Among recent publications on American birds, the three reviewed here deserve special mention, two of them for the new material they contain, and one for its illustrations.

Mayfield's volume *The Kirtland's Warbler*, is a unique monographic study, unique in that it presents a study of the entire population of a species. This warbler has the most restricted breeding range of any North American song bird; it nests only in a few counties in the northern portion of the Lower Peninsula of Michigan and in 1951 had a carefully estimated total population of 1000 adult individuals. It is apparently a relict species now severely limited by its requirement of a specialized nesting habitat which exists in a gradually diminishing area. In addition to this, the parasitic brown-headed cowbird has recently moved into the area and has become a very serious problem to the warbler, which has no adequate defense. In other words, we have here what may be a disappearing species and one which is certainly in a critical state. Mayfield presents a meticulous account of the history, distribution, and life history of this bird; he includes a special study of the cowbird in its relations to the Kirtland's warbler and a closely reasoned quantitative appraisal of its reproduction and mortality. All through the book one finds a most welcome mathematical approach, but an approach that probably can be attained only in treating a compactly situated species of very limited numbers.

Skutch's volume, the second in a series, presents a wealth of new observations on the vireos, warblers, thrushes, wrens, titmice, jays, swallows, and flycatchers of Central America. This is pioneering work, dealing with pro-

longed and careful field studies of a very rich avifauna previously known largely from the casual notes of collectors of specimens. Unlike so many pioneering efforts, Skutch's work has all the academic finish of the best modern studies of well-known species and has the advantage of presenting the author's interesting and penetrating thoughts and judgments, based on wide reading, together with his new factual material. No one entering upon a serious study of any of the North American species of the families covered in this book can afford to neglect this opportunity to become familiar with the knowledge of their tropical relatives, so ably recorded here on the basis of Skutch's prolonged residence and devoted work in Central America.

The third volume *Bird Portraits in Color* is illustrated with 92 colored plates, first issued in Roberts's two-volume work *The Birds of Minnesota*; it is a revised edition of an earlier presentation of the plates with a much abridged version of the text that Roberts had prepared for his larger work. W. J. Breckenridge, D. W. Warner, and R. W. Dickerman revised the text. The present book is primarily a picture book of some 295 North American bird species, but it gives enough carefully condensed information to be a reliable, easy reference work for the amateur bird student.

HERBERT FRIEDMANN
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Basic Values of Western Civilization. Shepard B. Clough. Columbia University Press, New York, 1960. x + 132 pp. $3.

This eloquently written small book, by a historian at Columbia University, represents a pioneering attempt to describe systematically the basic values of Western culture. The enterprise grew out of a Columbia University faculty seminar devoted to the content and methods of the social sciences. When the seminar turned to the question of the basic values of a culture, Clough collaborated especially with the anthropologist John P. Gillin in the application of the concepts of "culture" and "values" to a problem with which he had long been concerned: the role of ideologies and ideals in shaping human behavior through time.

After a brief discussion of concepts and methods, Clough moves immediately and boldly to a very large canvas —the whole of Western culture—and describes our basic values in five chapters: "The end of man is man"; "Societal institutions as basic values"; "Material values"; "Knowledge, religion, and aesthetics"; and "The glorification of progress." The book then closes with a chapter on variations in values and a chapter on the future of Western values.

Clough's discussion of methods is clear and succinct, and most anthropologists would, I think, also go along with his definitions of concepts and theories, except for one major point. He states that "in every culture the way of life of people is determined most basically by a set of values concerning goals to be attained" (page 2). While most anthropologists think that values are one of the determinants of culture, few would agree that the value system is always the most basic determinant. Indeed, there is much evidence to the contrary, especially in cultures with a hunting and gathering economy where the patterns are clearly determined most basically by ecological and economic conditions.

I find his chapters on the basic values to be somewhat uneven in approach, interest, and quality. His treatments in "The end of man is man" and the "Glorification of progress," are masterful summaries of these two key values. I am less impressed with the other chapters, and his description of societal institutions (that is, family, school, church, and nation-state) does not appear to me to advance the subject matter much beyond what one can read in an introductory textbook on sociology. I am also skeptical when he states "the social scientist can hardly resist the temptation to point the way our society seems to be moving and to suggest action which would turn it in the direction in which he would like to see it move" (page 118). Many of us feel we are several decades short of having the kind of social-science knowledge we need to make such predictions and suggest such action.

On the other hand, I agree with Clough's basic point that we in the West have been far too inarticulate about our basic values and that it is high time we studied them and made them more explicit. I hope he will persist in his efforts to find time and financial support "to write a long and heavily documented work based on extensive field work and intensive personal interviewing" (page viii), for in so doing he will undoubtedly answer the critical points I have raised. In the meantime, the present book is an interesting addition to our accumulating literature on the value systems of cultures.

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Advances in Fluorine Chemistry. vol. 1. M. Stacey, J. C. Tatlow, and A. G. Sharpe, Eds. Academic Press, New York; Butterworths, London, 1960. vii + 203 pp. $8.

In this era of accelerating increase in scientific research and its associated increase in the number of publications and the mediums of publication, it is extremely helpful to have well-written, comprehensive, and critical surveys or reviews, bringing together scattered information under logical canopies. The chemistry of fluorine and fluoro compounds, because of the extreme properties of the element itself or those of its compounds, requires a niche of its own in the chemical literature. That it has achieved this niche is evident, for the first international conference on fluorine chemistry was held a year ago at the University of Birmingham in England. The editors of this series were the organizers of that conference, and they have by their own research, contributed greatly to this field in which they have an enthusiastic interest. A good portion of this first volume was written by them.

The volume reviewed here is the first of a proposed new series which will, if continued in the excellent style of this initial volume, greatly advance the development of the field, and delineate the position of fluorine chemistry. This first volume covers much of the inorganic chemistry of fluorine and of the methods of fluorinating organic compounds. The subjects covered are: "The halogen fluorides—their preparation and uses in organic chemistry," by W. N. R. Musgrave; "Transition metal fluorides and their complexes," by A. G. Sharpe; "Fluoroboric acids and their derivatives," by D. W. A. Sharp; "The electrochemical process for the synthesis of fluoro-organic compounds," by J. Burdon and J. C. Tatlow; and "Exhaustive fluorinations of organic compounds with high valency metallic fluorides," by M. Stacey

and J. C. Tatlow. All the articles digest a relatively voluminous literature into well-written, readable, and concise discussions.

However, there is a vast area for future volumes in this series, and fluorine chemists will be looking forward to the appearance of each succeeding volume. The series is certainly to be recommended to workers in the field and to chemists in general.

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Readings in the History of American Agriculture. Wayne D. Rasmussen, Ed. University of Illinois Press, Urbana, 1960. xi + 340 pp. Illus. $6.50.

God Speed the Plow. The coming of steam cultivation to Great Britian. Clark C. Spence. University of Illinois Press, Urbana, 1960. 183 pp. Illus. $4.75.

Power to Produce. Yearbook of Agriculture, 1960. U.S. Government Printing Office, Washington, D.C., 1960. 480 pp. Illus. $2.25.

Harvests and Harvesting through the Ages. Norman E. Lee, Cambridge University Press, New York, 1960. viii + 208 pp. Illus. $2.95.

Readings in the History of American Agriculture contains some of the sources and documents of American agricultural history. The 52 selections include such items as a 16th-century account of Indian farming, a description of Jethro Wood's patent application for his plow, and analyses of how World War I affected agriculture and how artificial breeding altered dairying. Nearly every important aspect of American agricultural history is covered. Short, informative essays introduce the selections and give the background to, and the results of, the events covered by the documents. Unfortunately, and perhaps unavoidably, the essays do not always provide a continuous narrative link between the documents. Furthermore, many odd or archaic technical terms might have been defined in footnotes. Except for the 16th- and 17th-century documents, however, the illustrations help make things clearer. A fairly comprehensive chronology of American agriculture is at the back of the book. This generally useful outline also provides some continuity for the whole collection. The book emphasizes the scientific and technological development of American agriculture.

In *God Speed the Plow*, Clark Spence traces the efforts of Britons to plow and cultivate with steam-powered machines. The book covers the years from 1618 to 1918, but most of the story takes place in the 19th century. Methods of using steam power centered on (i) stationary engines, (ii) traction engines, and (iii) tractors with powered earth-cutters. Of these, the stationary engines proved to be the most practical. In Britain, at least, stationary engines always outnumbered tractors about 10 to 1. Spence's account of success and failure is told in adequate detail. He does, however, slight the general technical and economic conditions of the times. Thus, the reader may wonder why a 10 horsepower engine had to weight several tons, and why, even so, it might be profitable to own one. Otherwise, this is a thorough, interesting, and abundantly illustrated monograph.

Power to Produce, a yearbook of the U.S. Department of Agriculture, concentrates on present ways of using power, particularly power derived from internal combustion engines and electric motors. The book also covers some of the history of these devices. Although it is a collaborative effort, the book is still remarkably even in style. It appears to have been written primarily, but not exclusively, for an urban audience. Farm methods and machines are explained in detail. The authors intentionally make little effort to cover any other scientific practices or discoveries. The essays tend to repeat information given in other chapters, but this is probably unavoidable. Although the book can be read straight, it was apparently designed as a reference, to be read piecemeal. How does a potato digger work? How did the tractor evolve? Without using jargon, the authors answer these and a host of other questions. Altogether excellent.

The brief *Harvests and Harvesting through the Ages* was written primarily for children (12 to 16 years of age), and I recommend it for them. It is not a childish book, however, and might be read by adults who want a review of world agricultural history. The book covers far more than just harvesting, although it deals mostly with grain production and processing.

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Paléontologie Stratigraphique. Henri and Geneviève Termier. Masson, Paris, 1960. 515 pp. Illus. NF. 148.

Atlas de Paléogéographie. Henri and Geneviève Termier. Masson, Paris, 1960. 99 pp. Illus. NF. 16.

The indefatigable team of Henri and Geneviève Termier continues to turn out an almost incredible amount of synthesis and compilation in the field of historical geology, taken very broadly. Three volumes of their *Traité de Géologie* have appeared, and others are in preparation. In the meantime they have presented some of the same material in other forms, or for different audiences, and are making related but distinct compilations in several fields. Their *Paléontologie Stratigraphique* is a major work in itself. The purpose, translated from their own words, is "to give the reader an exact picture of the vegetable and animal population of our planet in each of the geological periods that have elapsed since about 500 million years ago."

For each geological period or (in the Cenozoic) epoch a table of its subdivisions is given, usually at the level of stages in European terminology, and its life is summarized in words. The floras and faunas, first marine and then terrestrial as appropriate for the given period, are then reviewed succinctly, telegraphically as the authors say, but with much detail, usually to families and often to genera. Each chapter ends with tables of paleontological zones, presented in terms of different groups of organisms and for various parts of the world. As each major group appears in geological sequence, it is taxonomically characterized and summary classifications are sometimes, but not consistently, given. The illustration is extremely rich, with more than 3425 different figures, mostly line cuts of individual fossils redrawn from innumerable primary and secondary sources. Little attention is given to detailed anatomy, and comparatively few figures have anatomical labels.

A book should be judged, first of all, in the light of its authors' intentions. Some errors of fact and many disputable points of opinion are quite unavoidable in an undertaking so very complex, but it can be said that the authors have carried out their own intention well. The extent to which the result corresponds with a felt need depends on each prospective reader. For some the book may seem to fall between two stools: neither a fully ade-

quate professional manual nor an easily grasped nonprofessional survey.

The *Atlas de Paléogéographie* has been issued as a sort of supplement to *Paléontologie Stratigraphique*. It consists essentially of the series of 35 paleobiogeographic world maps originally published in 1952 in the authors' *Traité*, with a new summary verbal description of each map. Many geologists will welcome the opportunity to obtain these maps in convenient and less expensive form. Nevertheless, it does seem fair to criticize the intention, not so much of these particular authors as of this whole approach toward teaching historical geology. In the present state of this science, an attempt to draw a conventional geographic map for any considerable part of the world during a remote geological epoch often involves quite as much fantasy as fact. For a world map, even the most fundamental points of geographic coordinates for crustal segments are still hotly disputed. Would it not be better to present to students (and to popular readers) a consensus of actual knowledge and reasonable inference? The sort of thing that could be done without going beyond the facts and their scientifically established integration is suggested by the sets of paleotectonic maps published by the U.S. Geological Survey. Apart from the general weakness of the conventional geographic approach, for which the Termiers have abundant precedent, their attempt to include biogeographic data is unfortunate. What is, and very likely all that could be, shown in this format is so inadequate and so erratic as to be more misleading than instructive.

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New Books

Economics and the Social Sciences

Community Resources in Mental Health. Reginald Robinson, David F. DeMarche, and Mildred K. Wagle. Basic Books, New York, 1960. 461 pp. $8.50. The fifth of a series of monographs to be published by the Joint Commission on Mental Illness and Health.

Crossroads. Land and life in Southwest Asia. George B. Cressey. Lippincott, Philadelphia, 1960. 607 pp. Illus. $12. A comprehensive geography of the area, written around three principal ideas—the crossroads character of Southwest Asia, the role of water in its economy, and the way in which man is changing the landscape.

Culture in History. Essays in honor of Paul Radin. Stanley Diamond, Ed. Published for Brandeis Univ. Press by Columbia Univ. Press, New York, 1960. 1038 pp. $15.

Demographic and Economic Change in Developed Countries. A conference of the Universities–National Bureau Committee for Economic Research. Princeton Univ. Press, Princeton, N.J., 1960. 545 pp. $12.

Education and Manpower. Henry David, Ed. Columbia Univ. Press, New York, 1960. 342 pp. $5.

An Introduction to Physical Anthropology. M. F. Ashley Montagu. Thomas, Springfield, Ill., ed. 3, 1960. 787 pp. Illus. $14.50.

Methods of Regional Analysis. An introduction to regional science. Walter Isard. Technology Press, and Wiley, New York, 1960. 813 pp. Illus. $9.50.

Political Science. A philosophical analysis. Vernon Van Dyke, Stanford Univ. Press, Stanford, Calif. 1960. 250 pp. $5.

The Preindustrial City. Past and present. Gideon Sjoverg. Free Press, Glencoe, Ill., 1960. 365 pp. $6.75.

Social Systems. Essays on their persistence and change. Charles P. Loomis. Van Nostrand, Princeton, N.J., 1960. 360 pp. $6.50.

The Soviet Cultural Offensive. The role of cultural diplomacy in Soviet foreign policy. Frederick C. Barghoorn. Princeton Univ. Press, Princeton, N.J., 1960. 360 pp. $7.50.

On Thermonuclear War. Herman Kahn. Princeton Univ. Press, Princeton, N.J., 1960. 671 pp. $10.

Values, Ethics, and Esthetics. A selected bibliography. Ethel M. Albert and Clyde Kluckhohn. Free Press, Glencoe, Ill., 1960. 360 pp. $7.50.

Mathematics, Physical Sciences, and Engineering

Analog and Digital Computer Technology. Norman R. Scott. McGraw-Hill, New York, 1960. 533 pp. Illus. $12.75.

Annual Review of Physical Chemistry. vol. 11. Henry Eyring, Ed. Annual Reviews, Palo Alto, Calif., 1960. 595 pp. $7.

Controlled Thermonuclear Reactions. An introduction to theory and experiment. Samuel Glasstone and Ralph H. Lovberg. Van Nostrand, Princeton, N.J., 1960. 539 pp. Illus. $5.60. This volume was prepared under the auspices of the Office of Technical Information, U.S. Atomic Energy Commission.

Crystal-Structure Analysis. Martin J. Buerger. Wiley, New York, 1960. 685 pp. Illus. $18.50.

Edison Experiments You Can Do. Based on the original notebooks of Thomas Alva Edison. Prepared under the direction of International Edison Birthday Celebration Committee of the Thomas Alva Edison Foundation. Harper, New York, 1960. 139 pp. Illus. $2.50.

Elektronenbeugung. Theorie, Praxis und industrielle Anwendungen. Ernst Bauer. Moderne Industrie, Munchen, Germany, 1960. 233 pp. DM. 32.

Elementary Introduction to Nuclear Reactor Physics. S. E. Liverhant. Wiley, New York, 1960. 461 pp. Illus. $9.75.

Essentials of Earth History. An introduction to historical geology. William Lee Stokes. Prentice-Hall, New York, 1960. 512 pp. Illus. $8.75.

Fuel Cells. G. J. Young. Reinhold, New York; Chapman and Hall, London, 1960. 159 pp. Illus. $5.75. A symposium held by the Gas and Fuel Division of the American Chemical Society at the 136th national meeting.

Fundamentals of Signal Theory. John L. Stewart. McGraw-Hill, New York, 1960. 359 pp. Illus. $9.

Hydrodynamics of Oceans and Atmospheres. Carl Eckart. Pergamon, New York, 1960. 302 pp. Illus. $9.

Imperfections in Crystals. H. G. Van Bueren. North-Holland, Amsterdam; Interscience, New York, 1960. 694 pp. Illus. $16.75.

Introduction to Laplace Transforms. For radio and electronic engineers. W. D. Day. Iliffe, London; Interscience, New York, 1960. 190 pp. Illus. $5.50.

Lehrbuch der Theoretischen Physik. Georg Joos. Akademische Verlagsgesellschaft, Frankfurt am Main, Germany, 1959. 859 pp.

Noise Reduction. Prepared for a special summer program at the Massachusetts Institute of Technology. Leon L. Beranek, Ed. McGraw-Hill, New York, 1960. 762 pp. Illus. $14.50.

Physical Chemistry of Surfaces. Arthur W. Adamson. Interscience, New York, 1960. 643 pp. $12.75.

Principles of Control Systems Engineering. Vincent Del Toro and Sydney R. Parker. McGraw-Hill, New York, 1960. 700 pp. Illus. $14.50.

Reactor Analysis. Robert V. Meghreblian and David K. Holmes. McGraw-Hill, New York, 1960. 817 pp. Illus. $19.50.

Runnerless Molding. Ernest P. Moslo. Reinhold, New York; Chapman and Hall, London, 1960. 174 pp. Illus. $4.95.

Soviet Research in Geophysics. vol. 4, *Investigation of the Mechanism of Earthquakes* (English translation). E. N. Bessonova *et al.* American Geophysical Union and Consultants Bureau, New York, 1960. 209 pp. $7.50.

The Space Encyclopaedia. A guide to astronomy and space research. Dutton, New York, ed. 2, 1960. 288 pp.

Space Research. Proceedings of the first international space science symposium. Hilde Kallmann Bijl, Ed. North-Holland, Amsterdam; Interscience, New York, 1960. 1211 pp. Illus. $24. This volume contains approximately 100 papers and constitutes the proceedings of a symposium held at Nice in January 1960. The papers are concentrated on the results achieved in the 2 years since the first sputnik was launched.

Stabilization of Free Radicals at Low Temperature. Summary of the NBS program. Arnold M. Bass and H. P. Broida, Eds. National Bureau of Standards, Washington, D.C., 1960 (order from Supt. of Documents, GPO, Washington 25). 114 pp. $1.50.

Source Book of Astronomy. 1900–1950. Harlow Shapley, Ed. Harvard Univ. Press, Cambridge, Mass., 1960. 438 pp. Illus. $10.

Water Supply. Economics, technology, and policy. Jack Hirshleifer, James C. De Haven, and Jerome W. Milliman. Univ. of Chicago Press, Chicago, Ill., 1960. 390 pp. Illus. $7.50.

Reports

Blue Halite

Abstract. Remarkable deep blue and occasionally purple forms of halite (rock salt) have long been known. These occur associated with potassium minerals in the Zechstein basin of northern Germany, elsewhere in Europe, and in North America. In recent laboratory studies, data have been developed which confirm the hypothesis that this coloration in natural salt may be attributed to gamma-ray bombardment by associated potassium-40 from sylvite, accompanied or followed by structural deformation.

Natural clear halite cleavage plates were subjected to irradiation from a 1000-curie cobalt-60 source over a period ranging from 1 hour to 4 days. Amber coloration resulted, which showed an increase in intensity with the duration of irradiation. Optical absorption spectra of irradiated halite cleavage plates were then obtained with a Cary model 11 recording spectrophotometer (Fig. 1c). Colored halite samples were held in the sample holder by means of brass plates with 0.25-inch-diameter apertures. These openings allowed equal and reproducible light beams to pass through the colored sample plates for comparison with beams transmitted through colorless halite plates used for reference purposes.

The colored cleavage plates were then deformed by unconfined pressures ranging from 25,000 to 125,000 lb/in.^{2}, in increments of 25,000 lb/in.^{2}, followed by immediate exposure to artificial light. All irradiated cleavages thus exposed showed a rapid color change from amber to blue, becoming deep blue at the end of 15 minutes. Since different pressures did not noticeably affect the intensity of the blue, additional irradiated cleavages were submitted to lower forms of deformation with a vise, a mortar and pestle, and a light blow with a hammer. In all irradiated specimens thus deformed, the amber color rapidly changed to blue after exposure to light (*1*).

Irradiated specimens were stored in darkness under slight pressure in a small vise. After a few months these halite cleavage cubes were found to have changed from amber to light blue.

A color comparison was next made with the spectrophotometer between natural blue halite from Germany and artificially produced blue halite. A natural blue halite cleavage plate, 0.055 inch thick, was found to yield four absorption peaks (Fig. 1*a*). The most intense peak at 6400 A corresponds to blue. A less intense peak at 5300 A corresponds to green, an even less intense peak at 4300 A corresponds to orange-red, and a least intense peak at 3700 A falls in the ultraviolet range. The latter three peaks are relatively minor in intensity compared to the peak corresponding to blue. At the same time, to the unaided eye, the only apparent coloration is deep blue.

Artificially produced blue salt plates were examined with the spectrophotometer (Fig. 1*b*). A broad compound peak, which lies in the spectral range from 5700 to 6300 A, corresponds to blue and green. In addition, two minor peaks are suggested by shoulders at about 5300 and 4300 A. A fourth peak, which approximately conforms in shape to the 5700–6300 A peak, extends from 4000 A into the ultraviolet spectral range. It is significant to note that no F-band, at 4650 A, may be detected; this coloration, observed in irradiated halite before deformation, is essentially destroyed by the application of pressure and is replaced by a new coloration.

Colored halites were also examined in thick sections with the polarizing microscope. Natural blue salt is in part optically inhomogeneous, and both blue and purple are unevenly distributed in various intensities and shades. Lines of dark blue parallel to the (110) crystal planes occur in a lighter blue matrix, and one set of streaks often transects another set at right angles. Numerous bubble inclusions and crystallites of mineral impurities such as anhydrite are apparent. The blue coloration is intimately associated with patches of clear salt. These boundaries may be at times irregular and difficult to distinguish on account of blue shading which grades into colorless sections, but in some cases the boundaries are quite sharp and straight. Where the distinction is clear it appears that the color divisons are crystal boundaries, and it indicates a recrystallization process which has removed color from those portions now colorless.

Optically, artificial blue salt closely resembles natural blue halite. When artificial blue salt plates are allowed to stand exposed to artificial light for a few weeks, the plates bleach in irregular patches corresponding to the appearance of natural blue halite. Many of the boundaries between colorless and blue salt are straight and sharp, indicating a recrystallization process. The artificial blue salt shows slight anisotropism between crossed nicols, caused by the application of pressure in the synthesis process. Pressure results in lattice distortions causing light to be irregularly refracted to produce anisotropism.

A criterion for the recognition of coloration by irradiation is removal of the coloration by bleaching with heat,

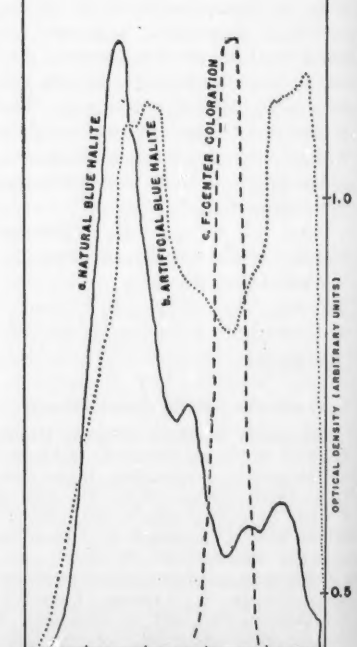

Fig. 1. Optical absorption spectra for halite: *a*, natural blue halite; *b*, artificial blue halite; *c*, F-center irradiation coloration in halite.

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

light, or solution (*2*, *3*). Natural and artificial blue salt, and unpressed amber salt, were bleached by heat, light, and solution to compare coloration properties. All apparent coloration was removed from all samples when dissolved in water. Cleavages 3 mm thick were heated over bunsen burners in crucibles with thermometers attached. One and one-half minutes at a final temperature of 150°C bleached artificial blue salt and unpressed amber salt. Two and one-half minutes at a final temperature of 220°C bleached natural blue halite. In natural blue halite, before all color bleached, a change to violet was apparent. At the end of two and one-half minutes a few blue streaks remained, oriented along diagonals to the cube faces, the known slip, or glide directions (110) caused by prior natural deformation. Such slippage would induce more intense deformation along diagonals and thus cause coloration to be more intense and more slowly bleached.

One 250-watt lamp was placed 15 cm from 3 mm-thick cleavages of natural blue salt, artificial blue salt, and amber salt to investigate bleaching by illumination. In 1 hour, the amber salt was bleached. In 8 hours, the artificial blue salt was bleached, although most of the bleaching occurred by the end of the first 3 hours. With the natural blue salt, no bleaching effects were noted after 35 hours.

Halite, upon gamma irradiation, application of pressure, and exposure to light in the order given, will change from colorless to amber and finally to blue. Long-continued minor pressure even in darkness will also turn irradiated salt blue.

Spectrophotometric examination of natural blue halite and artificially produced blue salt shows that the curves for the two agree in absorption peak positions and in the intensity of the predominant blue coloration. Figure 1 illustrates absorption spectra curves for natural blue halite, artificial blue salt, and amber F-center irradiation coloration.

Colorless halite from bedded deposits or from salt domes can be turned amber yellow to brown and even to black (F-center coloration) by gamma-ray irradiation from a cobalt-60 source. The intensity of color for a given irradiation interval is increased when specimens are subjected to confined or unconfined pressures prior to irradiation (*4*). Color intensity increases with exposure time, an increase of 14 times being noted between samples irradiated from 1 hour to 1 day. However, further exposure of 4 days yields an increase on the order of only two times that of a single day. This confirms that additional coloration proportionately decreases with longer irradiation times. Optical absorption spectra obtained with the spectrophotometer indicate that pressure causes shifts of color center band maxima, removal of some color centers, and development of more complex color centers (*5*).

Application of pressure prior to irradiation enhances the F-center coloration, but pressure applied after irradiation produces a change to blue. In this change to blue all amber F-center coloration is destroyed.

Gamma irradiation of natural blue halite does not appear to produce F-center coloration since spectrophotometric curves taken before and after irradiation for 1 hour are identical.

It would appear reasonable to infer that blue halite may be caused by natural gamma irradiation followed by deformational rock pressures (*6*). This interpretation is supported by the reported occurrence of blue halite in areas of deformation, such as accompany faults, shear zones, and contorted strata (*2*). Study indicates that even minor earth pressures may be adequate to yield blue in naturally irradiated salt (*7*).

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An Inherited Male-Producing Factor in Aedes aegypti

Abstract. An inherited factor causes a predominance of males in certain strains and in progeny of single pairs of *Aedes aegypti* L. This factor appears to be transmitted only by males and is not due to differential mortality, at least in postgametic stages. Mass release of male-producing males might be used in control operations.

With few exceptions, data on the sex ratio in mosquitoes have been collected only as a part of investigations of other phenomena. For some species, conflicting reports have given figures ranging from equal numbers of the sexes to a strong preponderance of one sex. In *Culex pipiens*, Qutubuddin (*1*) found that apparent departures from a 1:1 ratio were not statistically significant. Such a ratio would be expected since Gilchrist and Haldane (*2*) have shown that maleness appears to be due to a single dominant factor (male M/m, female m/m) in this species. While no controlled studies of sex ratio have been made for *Aedes aegypti*, most reports indicate that males predominate [35 to 45 percent female (*3*)].

In a study of genetic variability in populations of *A. aegypti* (*4*), laboratory strains were found to differ with respect to sex ratio. Among 16 strains of diverse origins, there were variations from 38 to 52 percent female. However, certain other strains were 18 to 32 percent female. Replicate counts on some strains over several generations indicated that the frequency of females was constant and predictable for each strain.

To investigate the abnormal ratios observed, frequency of the sexes was determined in the progeny of single pairs. Virgin adults were selected at random from laboratory strains having either high or low female ratios. Single-pair matings were made both within and between these strains. Repeated blood meals were offered to insure satisfactory egg production. Crosses giving fewer than 20 offspring, an infrequent occurrence, were not included in the results. In general, about 50 to 80 eggs were deposited after each blood meal, the total number of eggs from a cross depending upon the number of blood meals taken. Progeny in subsequent batches from any particular mating showed similar sex ratios. For example, a cross with 210 offspring gave 16, 12, 10, and 7 percent females in four egg batches. Since such variations were not statistically significant, the frequency of the sexes was computed from the total progeny produced in a cross.

Special efforts were made to discount the influence of mortality in the immature stages on the sex ratio. All eggs from each pair were collected, counted, and subjected to a hatching stimulus. Unhatched eggs were checked for embryonation. No correlation was observed between frequency of unembryonated or unhatched eggs and sex ratio. Newly hatched larvae were counted and carefully reared in uncrowded containers on a diet of liver powder. Pupation was usually completed by the 7th day after hatching. Sex ratios were determined initially on pupae and were later checked on newly emerged adults. All individuals from each egg batch were counted, even though a few required more than 7 days for pupation. Mortality levels for immature stages were below 10 percent

Table 1. The frequency of females in *Aedes aegypti* as determined in the progeny of single-pair matings made within and between certain strains. Only crosses yielding more than 20 progeny counted.

Strain	No. pairs	No. individuals in progeny	Frequently of females (%) Mean and S.D.	Range
Key West*	33	4648	48.9 ± 5.0	40–59
Texas†	21	2154	37.6 ± 11.2	11–56
Texas-D‡	30	2115	31.7 ± 15.5	4–66
Texas-D-S§	24	1724	22.5 ± 15.0	0–51
♀ Texas-D-S × ♂ Key West	15	889	47.9 ± 6.6	37–62
♀ Key West × ♂ Texas-D-S	22	2678	25.5 ± 11.6	6–49
♀ Key West × ♂ F_1 (♀ Key West × ♂ Texas-D-S)	23	5987	37.7 ± 9.3	15–53

* Field collected by A. O. Lea, Key West, Fla., in November 1959.
† Received from D. W. Micks, Univ. of Texas Medical School, 1957.
‡ Selected for dark larval color by inbreeding from the Texas strain followed by subsequent mass rearing.
§ Inbred line from the Texas-D stock selected for the male-producing factor.

for most of the data reported. There was no correlation between immature mortality and abnormal sex ratios. In a typical example, one female deposited 99 eggs containing embryos. Of these, six failed to hatch and 90 reached the adult stage, only four of these being females.

Table 1 and Fig. 1 show the percentage of females in offspring from single pairs. The results obtained from the Key West strain approximate a normal probability curve with a mean of 50 percent female. This strain resembles many other "normal" strains where a 1:1 sex ratio prevails. In the Texas strain both the mean of 37.6 percent and the range of 11 to 56 percent female indicate factors which cause a departure from the "normal" 1:1 ratio. This preponderance of males has been observed in other strains (*4*).

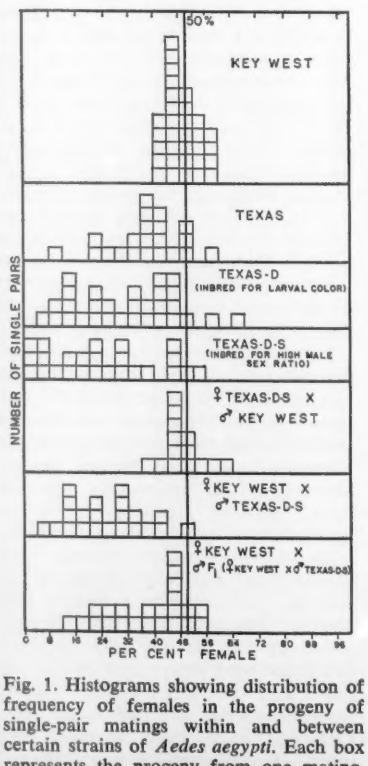
50%
KEY WEST
TEXAS
TEXAS-D (INBRED FOR LARVAL COLOR)
TEXAS-D-S (INBRED FOR HIGH MALE SEX RATIO)
♀ TEXAS-D-S X ♂ KEY WEST
♀ KEY WEST X ♂ TEXAS-D-S
♀ KEY WEST X ♂ F1 (♀ KEY WEST X ♂ TEXAS-D-S)
NUMBER OF SINGLE PAIRS
0 8 16 24 32 40 48 56 64 72 80 88 96
PER CENT FEMALE

Fig. 1. Histograms showing distribution of frequency of females in the progeny of single-pair matings within and between certain strains of *Aedes aegypti*. Each box represents the progeny from one mating. Each pair had at least 20 offspring and most had 90 or more offspring.

Certain stocks were derived from the Texas strain. The Texas-D stock was developed by inbreeding with selection for dark larval color. After five generations of single-pair sib matings, the stock was expanded by mass crossing. At this time it was observed that the mean had dropped from 37.6 to 31.7 percent female. Apparently, selection for one character led to enhancement of another. These results indicated the possibility that selection for abnormal sex ratios could further change the frequency of the sexes within a strain. To test this hypothesis, pairs from the Texas-D stock were mated and their progeny were counted for sex ratio. The progeny from pairs yielding less than than 15 percent female were inbred further. This inbred line constitutes the Texas-D-S stock. The selection process brought about a further reduction of the mean from 31.7 to 22.5 percent female. The fact that sex ratios can be changed by selection indicates that an inherited factor(s) is involved. While examples of this sort are well known in *Drosophila*, no factor causing abnormal sex ratios has been reported for mosquitoes.

Subsequent crosses between strains with high and low frequencies of females have demonstrated that the abnormal sex ratios are due to a factor here designated as the male-producing (*MP*) factor. In crosses between females from a male-producing line (Texas-D-S) and males from a strain in which the sex ratio is 1:1 (Key West), the F_1 showed equal numbers of males and females. This "normal" ratio is still evident when either F_1 males or females are backcrossed to Key West. Conversely, most of the crosses between "normal" females (Key West) and males from a male-producing line (Texas-D-S) showed a high male ratio in the progeny. When F_1 males from this cross are backcrossed to Key West females, the high male ratios are still evident.

Note that progeny from males carrying the male-producing factor are usually, but not always, MP themselves. A few sons from these males had 1:1 sex ratios in their progeny. However, the factor does not appear to be present in males of these families. In a typical case, a group of male sibs from an MP father gave 14, 22, 48, and 66 percent females. The males from the 14 percent family, upon inbreeding, gave 3, 9, and 23 percent females, whereas the males from the 48 percent family gave 33, 35, 36, 43, 45, 51, and 54 percent females. Experiments currently under way should make it possible to determine whether the male-producing factor can be recovered from these "normal" lines.

Certain conclusions can be drawn concerning the male-producing factor. First, it is inherited. Its frequency in a strain can be increased by selection. Moreover, the sex ratio in progeny from any particular cross is constant, as shown by data from subsequent egg batches. Second, data at present suggest that the factor is transmitted only by males. Third, it does not act through selective mortality of the sexes, at least at the postgametic level. Fourth, it is probably scattered through many strains and populations of *A. aegypti* (*4*). This may account for the conflicting data in the literature concerning sex ratio in this species.

Numerous problems concerning the male-producing factor remain to be solved. Paramount among these are the questions of its mechanism of action and its nature of inheritance and, for that matter, the sex determination mechanism for the species. The male-producing factor may act through abnormal spermatogenesis, resulting in selective production of male-determining sperm, or it may act through competition of sperm during fertilization. Furthermore, results with the factor are not entirely predictable, since males from MP-selected lines give batches of progeny varying from 0 to 50 percent female. Lines are being inbred in an attempt to stabilize the factor. This is difficult because of the paucity of females and the accumulation of sterility or lethal factors in these lines.

It is of interest to speculate on possible utilization of the male-producing factor in mosquito control programs. Perhaps breeding methods could be developed for mass rearing of male-producing males. Periodic releases of MP males in urban areas might reduce the number of females below the level required for efficient disease transmission.

In areas where resistance to chlorinated hydrocarbon insecticides is widespread, such a program would be especially effective if the males released also carried genes for susceptibility. Naturally, these hypotheses need to be tested in population cage experiments in the laboratory as well as in field populations (*5*).

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15 August 1960

Mechanism of Fibrillation Potentials in Denervated Mammalian Skeletal Muscle

Abstract. This report concerns the origin and mechanism of fibrillation potentials. It is proposed that these potentials do not necessarily arise from degenerated end-plate organs. The precursor is the rhythmic oscillations of the membrane potential. This mechanism may also occur in the case of epileptic discharges from epileptic cells in the cerebral cortex.

An excitable element, such as a nerve cell or a muscle fiber, is said to possess the property of spontaneous activity, if it is capable of discharge at a time during which no electrical energy is supplied from outside (*1*). In denervated skeletal muscle fiber, spontaneous rhythmic fibrillation potentials can be detected. The clinical significance of fibrillation potentials has been repeatedly discussed. This report deals with the underlying mechanism by which these fibrillation potentials are produced. The conclusions presented here stem from three series of experiments previously published.

Two questions may be asked: (i) Do the spontaneous rhythmic fibrillation potentials originate from a specific focal area in the muscle fiber? (ii) Is there any difference in the membrane properties of intact and denervated muscle fibers? The following experiments were performed in order to answer these questions.

The first series of experiments (*2*) was made under hypothermia on rats in which the electrical activity was recorded from the anterior gracilis muscle with intact motor nerve supplies. The second series of experiments (*3*) was made on rats in which the sciatic nerves had been severed 3 to 21 days prior to experimentation. The third series of experiments (*4*) was made on tissue cultures in which embryonic skeletal muscle fibers of the chick were isolated. Glass micropipette electrodes, recording intracellularly from single muscle fibers, were used in all of the experiments. The recording electrode, with a tip measuring approximately 0.2 μ in external diameter, was filled with electrolyte solution and connected by a platinum wire to a cathode follower, then to a direct-coupled amplifier, and finally to an oscilloscope.

In the first series of experiments, in which the motor nerve supplies to the rat muscle fibers were intact, the resting membrane potentials were found to be extremely stable. When the intracellular electrode impaled a muscle fiber near the motor end-plates, miniature end-plate potentials (*5*) were recorded. They were thought to represent asynchronous depolarization of small and restricted areas of the muscle membrane by a release of packages of acetylcholine molecules from the motor nerve endings (*6*). The discharge of these miniature end-plate potentials, presumably arising from different areas, could be synchronously evoked by applying a stimulus to the motor nerve. The synchronous discharge of many miniature end-plate potentials gave rise to an end-plate potential; if the end-plate potential exceeded a certain depolarization level, a spike discharge resulted. The evoked spike discharge seldom repeated itself; thus the response to the stimulus was almost always one-to-one. After the subsidence of the spike discharge, the resting membrane potential resumed its previous level and remained stable. No spontaneous rhythmic spike activity was recorded from the muscle.

In the second series of experiments with denervated skeletal muscle fibers, no miniature end-plate potentials were recorded. However, oscillation of the muscle membrane was often found. The oscillating potentials differed from the miniature end-plate potentials in that they were not all-or-none and random but were graded and rhythmic. If an oscillating potential exceeded a certain magnitude, a spike discharge or a series of rhythmic spike discharges was set off. The frequency of the rhythmic spike discharges was fairly constant in recordings from any given fiber; but occasionally a spike was missing and was replaced by an oscillation of potential. Furthermore, when the membrane potential of a muscle fiber was relatively stable, and when an electrical stimulus was applied to the fiber, the stimulus frequently precipitated a series of rhythmic spike discharges or oscillations. This is different from the responses of muscle fibers with intact motor nerve supplies.

In the tissue culture experiments, series after series of rhythmic activity synchronous with the fibrillary movements of the muscle fiber observed through the microscope were recorded by the intracellular electrode. This spontaneous activity was similar to that recorded from denervated skeletal muscle fibers and consisted of oscillating and spike potentials. The rhythmic oscillations occurred independently but gave rise to the rhythmic spike discharges. Therefore, they were the precursor of these spike discharges. Upon stimulation the cultured muscle fiber responded with a series of rhythmic spike discharges similar to the response of denervated muscle fibers. The response was sometimes a series of rhythmic oscillating potentials superimposed on a sustained depolarization following an initial spike discharge.

Earlier experiments with axons of the squid or lobster showed that oscillating potentials could be produced by sustained membrane depolarization (*7*, *8*). An increase in the oscillation of potential in axons of the squid and cerebral cortical neurons of the cat could be produced readily by veratrine and strychnine (*9*, *10*). These observations were obtained even when no electrical energy was supplied from outside the system. The spontaneous rhythmic spike discharges followed a decrease in the membrane potential and an increase in the oscillating potential.

In denervated mammalian skeletal muscle fibers, Ware, Bennett, and McIntyre also found a decrease in the resting membrane potential (*11*). They reported that the resting membrane potential dropped from a mean value of about −100 to −77 mv 22 days after denervation. Although their findings need to be confirmed (*3*), they are in keeping with the observations that sustained depolarization was accompanied by membrane oscillations or rhythmic spike discharges in nerves of the squid (*7*), lobster and crayfish (*8*, *12*), visual cells of the limulus crab (*13*), and cortical neurons of the cat (*10*).

It is known that a tropic change occurs in muscles deprived of motor nerve supplies. After denervation, an increase in hexokinase and a decrease in cytochrome oxidase activity were observed (*14*). Other metabolic changes remain to be found.

This study of denervated skeletal muscles in vivo and embryonic muscles in tissue culture explants indicates that no presynaptic activity is required for the production of rhythmic fibrillation

potentials. Furthermore, no neuronal elements or end-plate organs were found in the cultured muscle. The spontaneous rhythmic discharges must arise at some point along the muscle fiber itself.

The above observations would suggest that the presence of a degenerated end-plate organ in denervated muscle is not important for the generation of fibrillation potentials. It is likely, as found by McIntyre and his associates, that a decrease in resting membrane potential occurs as a result of a change in muscle metabolism following denervation. The decrease in resting membrane potential leads to instability and oscillation of the membrane potential and, finally, to rhythmic spike discharges. It is probable that this sequence of events also occurs in the case of epileptic discharges from pathological nerve cells in the cerebral cortex (*15*).

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Preparation of Monolayer Cell Cultures from Tissues of Some Lower Vertebrates

Abstract. Cold trypsin dispersion at *p*H 7.2 was used to obtain cultivable cells and cell groups from tissues of six species of fresh-water bony fishes, a frog, and a turtle. The cells readily attached to glass and were capable of at least limited, and in some cases extended, division in media consisting of commercially available components.

There are few fields of biological interest that have not found some use for the rapidly advancing techniques of cell and tissue culture. Paul's recent text on tissue culture (*1*) describes cultivation of tissues from cold-blooded animals, but the paucity of information reflects the scant attention that has been devoted to in vitro cultivation of tissues from the lower vertebrates. Madin *et al.* (*2*) reviewed the cultivation of tissues of domestic and laboratory animals but actually discussed only mammals. The recently compiled bibliography of tissue culture (*3*) contains more than 15,000 entries, but less than 0.002 percent deal with fish or fish tissue. Wolf and Dunbar (*4*) cited the principal works on fish tissue culture; all reports dealt solely with the technique of explantation.

The purpose of this report is to describe a method employed routinely in this laboratory during the past 9 months to obtain monolayer cultures of cells from several tissues of propagated trouts. Exploratory work with tissues from other easily obtained fishes, an amphibian, and an aquatic reptile has shown that the method is equally efficient for obtaining cultivable cells from these other animals.

Tissues were dispersed with a modification of the cold trypsinization described by Bodian (*5*). Since trout tissue explants prospered in vitro only when maintained at a *p*H between 7.2 and 7.4, the final *p*H of the digestion mixture was buffered at 7.2. Dulbecco and Vogt's phosphate-buffered saline (*6*) was modified to contain 1.065 g of Na_2HPO_4 and 0.249 g of KH_2PO_4 and constituted 87.5 percent of the digestion mixture. The balance consisted of a 2.5-percent trypsin solution (10 percent) (*7*) and, because it seemed to benefit the cells, human serum (2.5 percent). Potassium penicillin G and streptomycin sulfate were added at the rate of 200 unit/ml.

One to several volumes of saline-washed, scissor-minced tissue were used for each 10 volumes of the digestion mixture. All tissues were obtained as cleanly and aseptically as possible and, throughout preparation, were kept in vessels on or near ice. Most tissue was used soon after removal, but on occasion trout tissue was held at 4°C for 24 hours before digestion.

Dispersion was effected at 4° to 6°C in fluted erlenmeyer flasks on magnetic stirrers which had separate rheostats and, therefore, did not heat the mixture. Cells obtained from the first hour's harvest generally gave poor results. In practice, the first hour's harvest was discarded and replaced with an equal volume of fresh digestion mixture. Depending upon the relative speed of dispersion, the process was continued for three or more hours. Best results were obtained when the time did not exceed 20 hours. Overnight treatment, which usually released from 10^5 to 10^6 or more cells per milliliter, became the routine procedure.

Cells were sedimented from the harvest fluid by cold centrifugation at 200*g* for 20 minutes. The resulting packs averaged 0.1 or more of the starting tissue volume. The cells were washed once in cold salt solution that contained 2 to 5 percent serum and were then suspended in the growth medium. Dilutions of 1:400 to 1:600 (1 to 3 $\times$ 10^5 cells per milliliter) were normally made, although considerably higher dilutions were possible. The initial *p*H was between 7.2 and 7.4, and static vessels were incubated routinely at either 12.5°C or, preferably, 19°C.

Mammalian-type media (*7*), used in all work, readily stimulated attachment and division of cells from fishes, a frog, and a turtle (Fig. 1). These results are in accord with the determinations of Phillips *et al.* (*8*), which first showed that, except for phosphorus, the inorganic constituents of trout blood are remarkably like those of human blood.

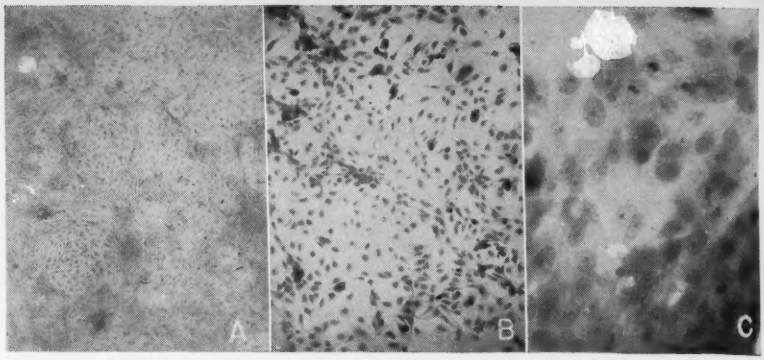

Fig. 1. *A*, Primary cultures of pooled gonad from immature rainbow trout (*Salmo gairdneri*) ($\times$ 24). *B*, Original cultivation of kidney from the bullfrog (*Rana catesbeiana*) ($\times$ 48). *C*, Original cultivation of ovary from the painted turtle (*Chrysemys picta*), showing cells in various stages of division ($\times$ 290). Hematoxylin and eosin stain.

Our results substantiate Paul's prediction (*1*) that media intended for mammalian cells would probably suffice for in vitro cultivation of cold-blooded animal tissue.

Cells of all the animals used grew in a medium consisting of 10 percent human cord serum (2 to 5 percent serum was suboptimal; a 20-percent level was unnecessary); 5 percent whole egg ultrafiltrate (chick embryo extract was contraindicated); 30 percent medium 199, 0.5 percent lactalbumin hydrolyzate, 10 percent Hanks' balanced salt solution, 45 percent Earle's balanced salt solution (which contained one-half the prescribed $NaHCO_3$), and antibiotics (400 units each of penicillin and streptomycin and 50 units of nystatin per milliliter). Melnick's monkey-kidney medium A or B, with an additional 8 percent calf serum, was adequate for initial attachment and growth of rainbow trout cells but was not tried on cells of other animals.

Soft-organ tissue was used in all trials, and a variety of results was obtained. Attachment and division of cells from testes of mature trout was consistently poor. Except for a largemouth bass (*Micropterus salmoides*), whose ovaries were infested with cestodes, ovarian or pooled immature gonadal cells of the animals tested readily attached and divided; good results were obtained for painted turtle (*Chrysemys picta*), rainbow trout (*Salmo gairdneri*), eastern brook trout (*Salvelinus fontinalis*), brown trout (*Salmo trutta*), bluegill (*Lepomis macrochirus*), and goldfish (*Carassius auratus*). Mature reproductive tissues of the bullfrog (*Rana catesbeiana*) gave poor results, but the kidney and to a lesser degree the heart cells were readily cultivated. In work with the rainbow trout, excellent monolayers were prepared from larvae. There was fair attachment and some mitosis among cells of the swim bladder, spleen, and kidney of the adult rainbow trout, but liver, heart, intestine, and gills gave poor results.

Initial attachment of cells of all animals began almost immediately, even in the hemocytometer. Depending upon initial density, cell dispersions from reproductive tissue of fish gave uniform monolayers of epithelial-like and spindle cells in 1 to 6 days at 19°C (Fig. 1). Monolayers of frog and turtle cells formed more slowly.

Subcultures of fish cells have been prepared also by mechanical dispersion, by a 10-minute cold digestion with 0.25-percent trypsin, but preferably by 10-minute cold dispersion with disodium versenate (20 mg/100 ml) followed by immediate "neutralization" with the medium in which the cells had been grown.

Subcultures of fish cells have been carried on the media described, on the growth medium of Puck *et al.* (*9*), and on media consisting of 10 percent human cord serum and either 90 percent Eagle's basal medium, Eagle's minimal essential medium, or NCTC-109.

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18 August 1960

Hormology in Nutrition

Abstract. Discrepancies in the growth curves for the sodium requirement of the cricket indicate that hormology may play an important role in the response to essential nutrients as well as to toxic compounds. Differentiation between stimulation of growth by a nonnutritive action and support of growth by an essential nutrient is difficult.

Discrepancies in data obtained during the determination of the quantitative requirement for sodium in crickets (*1*) suggested the possibility that this and other unexplained phenomena in nutrition may be due in part to hormology. Hormology has been defined as the study of stimulation and excitation (*2*). The general term includes hormoligosis, the stimulation resulting from any harmful agent present in minute quantities. Hormones are presently being studied as typical systems of hormology. Hormetics (*3*) are toxic compounds which excite in low concentrations. Many reactions of pharmacology show typical dose-response curves predicted by hormoligosis (*4*).

Multiphasic response curves were obtained by Richet (*5*) with concentrations of heavy metals which were 10 to 1000 times less than those which exhibit the harmful oligodynamic action reported by Nägeli (*6*). Schultz (*7*) and Branham (*8*) have shown that most of the classical bactericidal compounds stimulate bacteria when present in very dilute concentrations. Antibiotics sometimes exhibit this phenomenon. The myriad reactions reported for antibiotics in nutrition (*9*) cannot all be attributed to the intestinal microorganisms—for example, the decreased mineral requirement, or the changes seen in germfree plants (*10*) and animals (*11*). Other unexplained phenomena in nutrition include (i) the observation that certain derivatives of vitamin B_{12} are more active than the vitamin itself (*12*) and (ii) the increased growth in pigs that is attributed to copper sulfate (*13*). To this list may be added the reactions obtained by adding graded levels of sodium chloride to the diet of crickets. The dose-response curves obtained are extremely atypical for an essential nutrient, but they strikingly resemble the responses obtained in hormoligosis.

Day-old crickets, *Acheta domesticus* (*L*), were placed in glass or plastic specimen jars covered with cheesecloth. Each jar contained a crumpled 7-cm filter paper on the floor for cover; a cotton-stoppered 25-ml erlenmeyer flask with distilled water for drinking; and dry, powdered diet—grass (*14*) with graded amounts of sodium chloride—placed in one corner. The crickets were the offspring of several females and were all fed from the same mix. Sodium chloride solution was mixed into the grass with washed neoprene gloves. More grass was added to give the desired concentration of Na^+ in the food. The jars were placed at random in one tray of an egg incubator at 37°C and relative humidity of 65 percent. The crickets were weighed individually at 30 days, and the average weights were either plotted in milligrams or converted to relative weights (the weights of crickets fed unsupplemented grass was taken as unity).

The response to graded low levels of sodium chloride resulted in multiple stimulation peaks—a growth pattern which resembled that of hormoligosis rather than the smooth response curve expected for an essential nutrient. These data (Fig. 1) are quite similar to those obtained by Richet with different concentrations of salts of heavy metals in lactose fermentation. The two lower curves are based on data obtained in one laboratory; the two upper curves were derived in another laboratory. As the same batch of diet, the same hatch of crickets, and the same time periods were used in the two experiments, the differences are not easily explained. The results at 60 and 100 days were similar to those presented in Fig. 1. The data indicate an instability of the effect of sodium upon the growth of grass-fed crickets.

The experiment was repeated with fresh materials. Average results for 18 crickets (three separate cages of six

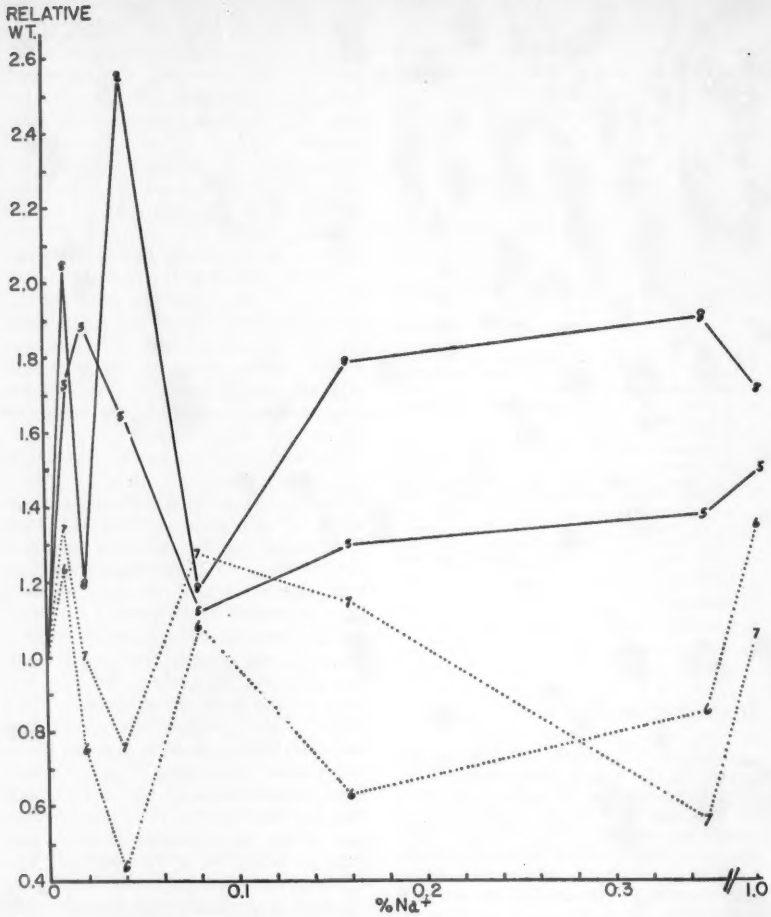

Fig. 1. Response of crickets to sodium chloride. Points 6 and 7 are average values from six crickets reared individually. Points 5 and 8 are average values from 12 crickets reared in two groups of six each.

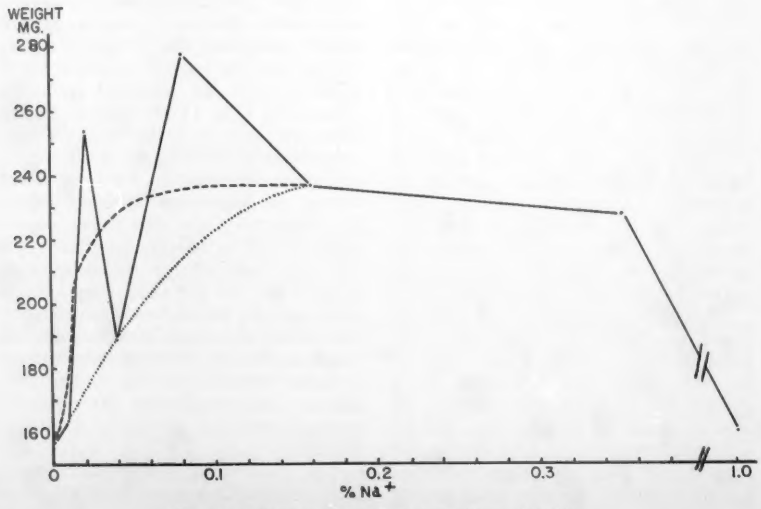

Fig. 2. Response of crickets to sodium chloride.

crickets each) are represented by each point in Fig. 2, to show the actual weight obtained with different amounts of sodium chloride added to the grass. The pattern is very similar to the results obtained previously. All the points above 200 mg are significantly different from the base at the 0.001 level. The weight of crickets fed 0.04 percent Na^+ is not significantly different ($p = 0.04$) from that of crickets fed unsupplemented grass. Crickets fed either 0.02 percent or 0.08 percent Na^+ were significantly heavier ($p = <0.001$) than those fed 0.04 percent Na^+. The amount of sodium in the unsupplemented grass was 0.023 percent in this experiment. The ratios of sodium found by analysis to added sodium are as follows: 0.030:0.01; 0.179:0.10; 0.329:0.35; and 0.882:1.00.

The sodium requirement of the cricket could be presented by the dotted line in Fig. 2. However, this ignores data which do not conform to the classical growth curve. The dashed line represents the median of all points and may approximate the actual requirement of Na^+ as an essential nutrient. But repeated observation of the peaks in the growth response with low levels of sodium chloride suggests that these are actually a part of the biological activity spectrum. The pattern is almost identical with results obtained by Richet (*5*) and could be interpreted as hormoligosis—the stimulation that is caused by a relatively small quantity of an agent which is harmful in large quantities.

An interpretation of hormoligosis by this particular agent, sodium chloride, poses two problems. First, while other agents of hormoligosis are clearly harmful (bactericides, heavy metals, antibiotics, irradiation, and so on), sodium chloride is harmful only in the sense that too much of any agent can be harmful. Most essential nutrients have known toxicities when taken in excess. The more important problem is how the hormetic action of an essential nutrient can be distinguished from its nutritive action. Attention must be given to detailed examination of the complete spectrum of the biological activity of nutrients at different concentrations. Presumably one concentration of a compound may stimulate some systems and depress others.

A possible generalized mechanism for hormoligosis has been presented previously (*9*). In the absence of information on water balance, body composition, food consumption, and food efficiency, a specific mechanism for the data presented cannot be given. This particular effect could be mediated through taste or smell as well as through other physiological mechanisms.

The importance of hormology in nutrition lies in the emphasis it places upon the concept that stimulation of one or more systems by a compound or element does not make that compound or element an essential nutrient (*15*).

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Hemoglobin Types of the Caingang Indians of Brazil

Abstract. We studied the hemoglobin types of 440 individuals (306 unrelated) cytologically and by paper electrophoresis. These included 334 (252 unrelated) putative "full bloods." No abnormal types were found.

A great amount of work has already been done concerning the distribution of the abnormal hemoglobins in several regions of the world (*1, 2*). However, the North and South American Indians and Eskimos have not been studied as intensely as other populations. In regard to South America, the only records to date of electrophoresis studies are the findings of Arends and Layrisse (*3*) of no abnormal hemoglobins in a small sample of 51 Carib Indians, and of Layrisse *et al.* (*4*) of one AS individual among 103 Guayqueri mestizos; both groups were from Venezuela.

The study of the possible occurrence of abnormal hemoglobins among these Indians, however, could be of great anthropological interest. For instance, hemoglobin E appears with relatively high frequencies among Mongoloid populations, but not among the Chinese (*1*). If the occurrence of hemoglobin E could be demonstrated among the Polynesians and other inhabitants of the Pacific, if its absence could be demonstrated in Siberian and North Mongolian populations, and if it occurred among the American Indians, we would have new evidence for Rivet's thesis (*5*) that the American aborigines originated, at least in part, from Pacific peoples. The study of the hemoglobin types of the American Indians could result, also, in the discovery of new forms that would only occur among them. These considerations led us to include this characteristic in a long-term study which is being performed among the Caingang Indians of Rio Grande do Sul, Brazil (*6*).

The populations studied live in four localities in the northern region of the state of Rio Grande do Sul. Blood was collected by venepuncture and placed in sterilized tubes, which were kept in an ice-box until arrival at our laboratory, 3 or 4 days after the collection. In the laboratory the samples were washed three times with saline, hemolyzed by repeated freezing and thawing, and centrifuged at about 1300 *g*. Afterward their concentration was colorimetrically adjusted to about 8 percent with Veronal. The separations were made in a horizontal paper electrophoresis chamber, with Whatman 3 mm filter paper, Veronal buffer, *p*H 8.6, ionic strength 0.05, field strength 4 volt/cm, and duration of 12 hours. Constant voltage was supplied by a Heathkit power supply model PS-3.

Emmel's cytological test (*7*) was performed also, which consisted in placing a drop of blood between a microscope slide and coverslip, and in isolating it from the air with a mixture of paraffin and wax. After 48 hours, the preparation was screened for the presence of sickle cells.

A total of 440 individuals were studied, approximately 100 from each locality; 334 of them were classified as "full bloods," and 106 as mestizos, on the basis of their morphological appearance and, in some cases, by interrogating them or other Indians about possible white or Negro ancestry. Of these, 252 "full bloods" and 54 mestizos were unrelated, as shown by detailed genealogical records. None of them presented any abnormal hemoglobin. In Table 1 our results are compared with those of several other authors. The findings in individuals with no signs of white or Negro admixture are generally negative, with the exception of the reports of Rucknagel (*8*) and Carbonell and Alemán (*9*). Rucknagel, however, believes that the Hb^{g} gene found in his sample can be explained by undetectable crosses with Negroes, and the same could be true for the two AS individuals found by Carbonell and Alemán.

In some individuals of our sample

Table 1. Abnormal hemoglobin surveys in North and South American Indians. *Methods.* (A) Paper electrophoresis and other biophysical methods; (B) paper electrophoresis only; (C) paper electrophoresis and cytological (reducing substance: metabisulphite); (D) cytological only; (E) cytological only, (reducing substance: hydrosulphite); (F) cytological only, Scriver and Waugh (*17*) method; (G) paper electrophoresis and cytological, Emmel (*7*) method.

Population	Method	Frequency of individuals with abnormal Hb	No. of individuals tested
Eskimos, Alaska, U.S.A. (*12*)	(A)	0	708
Indians, Alaska, U.S.A. (*12*)	(A)	0	44
Aleuts, U.S.A. (*12*)	(A)	0	200
Indians, Alaska, U.S.A. (*13*)	(B)	0	169
Several tribes, U.S.A. (*8*)	(C)		
Indians		AS(0.75)	133
Mestizos		AS(1.59)	126
Cherokee, North Carolina, U.S.A. (*14*)	(C)		
Indians		0	136
Mestizos		0	398
Lumbee, (Indians and mestizos), North Carolina, U.S.A. (*14*)	(C)	AS(1.73) AC(1.73) AD?(0.08)	1332
Caribs, Venezuela (*2*)	(C)	0	51
Guayqueri mestizos (*3*)	(B)	AS(0.97)	103
Serra de Perijá Indians, Venezuela (*9*)	(D)	AS(1.64)	122
Several tribes from Amapá, Maranhão and Mato Grosso, Brazil (*15*)	(E)	0	1379
Fulniô mestizos, Pernambuco, Brazil (*15*)	(E)	AS(1.81)	166
Guaranis, Nonoai, Rio Grande do Sul, Brazil (*16*)	(F)	0	47
Guarani mestizos, same locality (*16*)	(F)	0	5
Caingangs, same locality (*16*)	(F)	0	27
Caingang mestizos, same locality (*16*)	(F)	0	22
Caingangs, Rio Grande do Sul, Brazil (this report)	(G)	0	334*
Caingang mestizos, Rio Grande do Sul, Brazil (this report)	(G)	0	106†

* Of which 252 are unrelated † Of which 54 are unrelated.

we found increased amounts of the normal fraction A_2. Because of the interesting relations of this fraction with hemoglobin E and thalassemia (*10*), plans are being made for the study of this fraction in a quantitative and systematic way in future surveys (*11*).

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26 July 1960

Appearance of a Substance in Acute Phase Serum Which Elicits Cx-Protein Responses

Abstract. Seromucoid was isolated from serum of rabbits during the acute inflammatory period following subcutaneous injection of an irritant and at times after whole body irradiation. It produced a Cx-protein response on intravenous injection into normal rabbits, an anamnestic response in immunized rabbits, leucocyte changes characteristic of inflammation, and marked toxicity in rabbits with reticuloendothelial system blockade. Normal seromucoid did not possess these properties.

C-Protein (CP) and Cx-protein (CxP) are abnormal serum proteins which appear in serum of man and rabbit, respectively, in response to various noxious stimuli. They appear within 24 hours of application of a sufficient stress and disappear as rapidly with its termination. Their function is unknown, although C-protein has been reported to increase leucocyte mobility, and rabbits which produce high titered antibody give stronger Cx-protein responses than those producing lower titered antibody (*1*). Cx-protein apparently enhances the inflammatory tissue response to noxious agents (*2*). Their source and mechanism of appearance in serum is unknown although the reticuloendothelial system has been implicated (*3*).

A second abnormal substance(s) has been found in serum which appears coincidentally with Cx-protein 24 hours after subcutaneous injection of mineral oil-Aquaphor emulsion (incomplete adjuvant) and at various times after 500 r of whole body irradiation (*4*). We refer to it as Co-Cx-protein. It was first recognized by its ability to elicit a Cx-protein response on intravenous injection of small amounts into normal rabbits. Such activity was not associated with comparable fractions of normal serum.

Material with this activity is prepared by Winzler's method for the isolation of seromucoid (*5*) from the fraction of proteins which precipitate from acute phase serum with ammonium sulfate between one-half and three-fourths saturation. Table 1 summarizes the properties of a number of such preparations.

Table 1. Properties of various seromucoid preparations.

Source rabbits	Samples (No.)	Cx-protein titer* of serum source	Yield of seromucoid (mg/100 ml)	Dose injected in test rabbits† (mg)	Cx-protein titer of test rabbits 24 hr after injection
Normal	6	neg.–trace	3.5–6.7	0.35–2.1	5 neg., and 1:2
24–48 hr post adjuvant	4	1:8–1:16	13–21	1.2 –5.0	1:4–1:64
1 hr post irradiation	2	neg.	6.6–9.4	1.3 –1.9	1:4, 1:8
3 hr post irradiation	3	neg.–trace	5.4–9.7	1.1 –2.0	neg., 1:2, 1:2
6 hr post irradiation	3	neg.–1:8	5.8–16	1.2 –3.2	neg., neg., 1:2
12 hr post irradiation	3	1:2–1:8	13–19	2.5 –3.8	neg., 1:1
24–48 hr post irradiation	4	1:4–1:16	13–25	2.5 –5.1	1:1–1:8

* Cx-protein titers are recorded as the highest dilution of serum giving a positive test with specific antiserum in capillary precipitin tubes (*10*). † Usually the dose selected was equivalent to 20 ml of donor serum, although active samples were tested in lower doses and inactive samples at higher doses on repetitive tests.

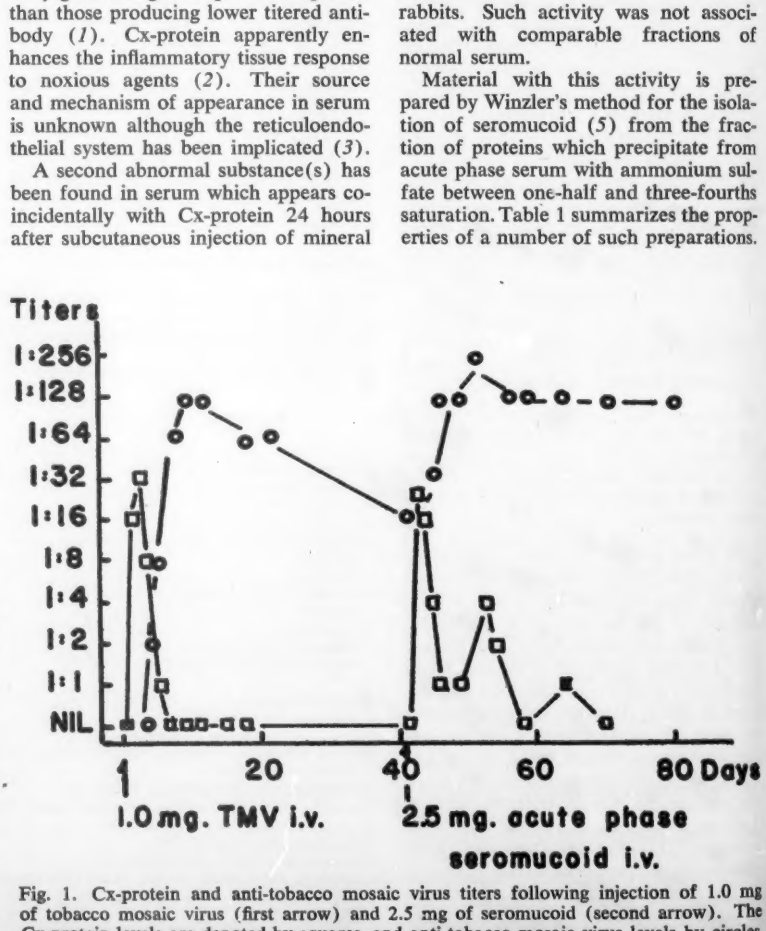

Fig. 1. Cx-protein and anti-tobacco mosaic virus titers following injection of 1.0 mg of tobacco mosaic virus (first arrow) and 2.5 mg of seromucoid (second arrow). The Cx-protein levels are denoted by squares, and anti-tobacco mosaic virus levels by circles.

As noted above, seromucoid from normal rabbit serum produced no Cx-protein response; that from serum obtained 24 to 48 hours after injection of adjuvant was uniformly active in this regard; and that from serum obtained 24 to 48 hours after 500 r whole-body x-irradiation was also active. Seromucoid obtained earlier than 24 hours after irradiation was irregularly active. Activity was commonly but not invariably associated with sera which gave the highest yields of seromucoid.

Other physiological activities of Co-Cx-protein have also been noted. Besides eliciting a Cx-protein response on intravenous injection, Co-Cx-protein elicited an anamnestic type of response on injection into the immunized rabbit. For example, in Fig. 1 is shown the Cx-protein response and the immune response of a rabbit given 1 mg of tobacco mosaic virus (*6*) intravenously, without adjuvant, followed on the 42nd day by 2.5 mg of Co-Cx-protein prepared from post adjuvant serum. Similar anamnestic responses to Co-Cx-protein have been obtained in rabbits immunized to BSA and employing Co-Cx-protein isolated electrophoretically, as well as to seromucoid prepared by Winzler's method. The absolute numbers of circulating lymphocytes were invariably decreased 50 to 75 percent in 4 to 6 hours following intravenous injection of seromucoid fractions which elicited a Cx-protein response. Very frequently a compensatory increase in heterophils also occurred to give an over-all leucocytosis. Another property of Co-Cx-protein which has been noted is its toxicity in the rabbit after blockade of the reticuloendothelial system with colloidal thorium dioxide (Thorotrast) (*7*). Injection of Co-Cx-protein 2 to 3 hours after intravenous injection of a nonlethal dose of Thorotrast was invariably fatal within 24 hours. Normal seromucoid did not enhance Thorotrast toxicity.

The activities associated with acute phase seromucoid could be ascribed to a new humoral factor, or to the appearance in this fraction of toxic tissue polysaccharides, or endotoxin. While we incline toward the latter view, more extensive comparisons are being made to establish this with certainty. With the possibility that endotoxin is released shortly after whole body irradiation and because of the marked enhancement of the toxicity of injected endotoxin administered during reversible hemorrhagic shock (*8*), it will be desirable to assess the toxicity of postirradiation serum in reversible hemorrhagic shock (*9*).

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12 August 1960

Zinc-65 and Chromium-51 in Foods and People

Abstract. Some radioisotopes introduced into the Columbia River via effluent water from reactors at Hanford, Wash., are found in crops irrigated with this water and in sea food harvested near the mouth of the river. Measurements of zinc-65 and chromium-51 in foods and in individuals consuming these foods are reported and are compared with zinc-65 concentrations resulting from fallout.

The use of water as a coolant for reactors at Hanford, Wash., with its subsequent disposal to the Columbia River, results in the introduction of several radioisotopes into the river. The half-lives of some of these are sufficiently long to make it possible to trace their distribution from the river through the produce of irrigated farms to man, and through the food chains of the aquatic life of the river (*1*). A report describing the distribution of zinc-65 in some produce from farms irrigated with Columbia River water, and in individuals utilizing these materials, was published earlier (*2*). Recently some Zn^{65} concentrations present in foods as a result of radioactive fallout were reported by workers in other laboratories (*3*). During 1959 and early 1960, a more comprehensive study of the Zn^{65} concentrations in produce from farms irrigated by the Columbia River and in locally available commercial sea foods was made. These concentrations have been compared with those found in foods from other locations in the world.

A NaI(Tl) scintillation well crystal detector 9⅜ inches in diameter (*4*) (which would hold a 500-ml polyethylene sample bottle) and a multichannel analyzer were used for the measurements, unless otherwise stated. The Cr^{51} concentrations were also determined, where possible, by direct gamma-ray spectrometric analysis. The samples of produce from farms irrigated with Columbia River water were obtained from an irrigation project about 30 miles downstream from the Hanford reactors. Samples of produce from two farms, one of which used sprinkler irrigation and the other ditch irrigation, were collected during the summer of 1959. The Zn^{65} and Cr^{51} concentrations observed in produce from these farms and in produce from other areas are tabulated in Table 1. Although it was not possible to obtain the same crops from the two farms for comparison, it is apparent that levels of Zn^{65} and Cr^{51} are generally higher in the crops from the sprinkler-irrigated farm, and that levels in the samples of tomatoes, carrots, green beans, corn, and alfalfa, which were obtained from both locations, were higher in samples from the sprinkler-irrigated project. This would indicate that direct foliate absorption through the leaves of sprinkler-irrigated crops is playing an important role in the uptake of Zn^{65} and Cr^{51}.

Studies at the Washington State Irrigation Experiment Station at Prosser have shown that soils in this area are deficient in available zinc (*5*). This condition could enhance the uptake of Zn^{65} by either method of irrigation.

The pasture grass listed with the produce from the ditch-irrigated farm (Table 1) has a much higher Zn^{65} content than grass from the sprinkler-irrigated farm, but this finding is undoubtedly related to the amount of irrigation, for this land was irrigated almost continuously. Milk samples were not available from this farm during the summer of 1959; however, those obtained during 1958 (*2*) showed about two to three times the Zn^{65} listed for the sprinkler-irrigated farm. The Cr^{51} concentrations listed in Table 1 are comparable with the levels of Zn^{65} in several of the crops, but Cr^{51} was not observed in milk and meat. The absence of Cr^{51} in milk and meat, plus its relatively short half-life, limits its availability to human beings. To date, Cr^{51} has not been positively identified in individuals from this area by total-body counting.

The individual who operated the sprinkler-irrigated farm and whose diet consisted mainly of the foods included in Table 1 was counted on 30 August 1959 in the Hanford whole-body counter [similar to that developed by Marinelli *et al.* (*6*)]. His Zn^{65} content was about 0.2 μc, and no Cr^{51} was observed. For comparison, 6 μc is the exposure limit recommended by the National Committee on Radiation Protection (*7*) for persons in the neighborhood of a controlled area.

The Zn^{65} concentration in milk from the sprinkler-irrigated farm was meas-

ured periodically from 16 July 1959 through 28 October 1959 and was found to average 1.9 $\mu\mu$c/ml. A family of six, including four children from 14 months to 8 years of age, which had consumed 1 gal of milk a day (average 0.0073 μc/gal, varying from 0.0045 to 0.0099 μc/gal) from this source for approximately 7 months, were counted in the total-body monitor on 12 October 1959. The milk was the only known source of Zn^{65} in their diet. The whole-body counter showed the average Zn^{65} content of these six individuals to be 0.047 μc, varying from 0.037 to 0.066 μc per person. The Zn^{65} content appeared to be related to the amount of milk consumed rather than to the age of the individual. Calculations, based on an effective half-life for Zn^{65} of 125 days for man (*8*) in the secular equilibrium equation, show that 31 percent of each day's intake of Zn^{65} must be retained in the body. This value may be somewhat low, since the fodder for the cows contained a smaller fraction of pasture grass during the spring months.

Table 1. Concentrations of zinc-65 and chromium-51 in farm produce (in $\mu\mu$c/g).

Sample	Zn^{65}	Cr^{51}
Sprinkler-irrigated farm (Columbia River water)		
Alfalfa	8.9	
Pasture grass	3.9	6.9
Soil (to depth of 2 in.)	5.6	38.4
Milk	1.9	<0.23
Corn	2.1	
Green beans	1.3	
Okra	0.59	
Carrots	0.36	1.0
Lettuce	0.45	1.2
Tomatoes	0.36	0.81
Boysenberries	0.29	0.67
Cantaloupe	0.28	
Cucumbers	0.28	
Watermelon	0.22	
Eggs	0.22	
Grapes	0.19	0.86
Cabbage	0.19	0.77
Zucchini squash	0.15	
Hamburger*	4.6	<0.23
Pork, flesh*	0.35	
Pork, fat*	0.27	
Pork, bone*	0.90	
Ditch-irrigated farm (Columbia River water)		
Pasture grass	36.0†	8.0
Wheat	4.0	0.81
Mint	2.8	
Corn	1.6	
Alfalfa	0.90	
Cabbage	0.28	
Carrots	0.17	0.29
Beets	0.16	0.14
Potatoes	0.14	
Green beans	0.095	
Strawberries	0.095	
Raspberries	0.090	
Onion tops	0.077	
Red beet tops	0.077	
Sugar beets	0.063	
Tomatoes	0.035	
Irrigation water	0.072	3.2
Other Locations		
Corn	0.016	Calif.
Meat (beef)	0.0090	Yakima, Wash.
Tomatoes	0.0072	Calif.
Green beans	0.0036	Calif.
Carrots	<0.002	Spokane, Wash.
Lettuce	<0.001	Calif.

* Calculated for the date the sample was received. † This pasture grass was irrigated almost continuously.

Zinc-65 is known to be a constituent of fallout, and it has been shown that Zn^{65} from fallout is present in people (*9*). To compare the levels of fallout-produced Zn^{65} in foods from other areas and in the produce from the Columbia River irrigation projects, foods from other localities were analyzed. The concentrations, listed at the bottom of Table 1, are lower by two to three orders of magnitude than those in samples from the local irrigation projects. The concentrations were measured by chemically separating the zinc from approximately 3-kg samples and counting it in a well crystal 5 in. in diameter and 5 in. high. If the Zn^{65} content of these foods is representative of foods in the diet of people of the western United States, it would be expected that this population would obtain less Zn^{65}, by two to three orders of magnitude, from farm produce than the individual whose diet consists of foods from the sprinkler-irrigated farm. Actual measurements with the whole-body counter of Zn^{65} in about 200 individuals, most of whom worked at the Hanford projects but were not exposed to Zn^{65} in any known way other than from fallout, showed body contents averaging about 0.0036 μc.

In the measurement of radioisotopes in people at Hanford it was found that the Zn^{65} in at least one individual could not be traced to farm produce from the Columbia River irrigation projects and was too high to be attributed to fallout. An examination of possible sources of this radioisotope revealed that oysters, which were a frequent part of the individual's diet, contained Zn^{65}.

In view of this finding, a program was set up to measure the Zn^{65} in sea foods from the local markets. About 50 samples of sea foods, including oysters, crabs, tuna, salmon, clams, shrimp, sardines, anchovies, codfish, herring, and halibut, were measured by counting them directly in the 9⅜-in. well crystal (see Table 2). The sample sizes varied from about 100 to 500 g, and the corresponding detection limit for the large crystal varied from about 0.05 to 0.25 $\mu\mu$c/g for 30-minute measurements. Of the 14 oyster samples measured, nine were found to contain measurable amounts of Zn^{65}. Except for samples 2 and 3, only the packing or distributor location is known, but all of the samples measured containing more than 0.5 $\mu\mu$c/g originated on the West Coast of the United States.

Table 2. Zinc-65 in oysters from local markets.

Sample No.	Packing	Origin	Zn^{65} ($\mu\mu$c/g)
1	Fresh	West Coast	63.5
2*	Fresh	Willapa Bay, Wash.	56.3
3†	Fresh	Willapa Bay, Wash.	37.8
4	Fresh	South Bend, Wash.	38.1
5	Canned	West Coast	15.1
6	Canned‡	Seattle, Wash	4.13
7	Canned	Gulf of Mexico	<0.1
8	Canned	New Orleans, La.	0.30
9	Canned	Biloxi, Miss.	<0.1
10	Fresh	Port Norris, N.J.	0.19
11	Canned	Japan	0.19
12	Canned	Japan	<0.2
13	Canned	Japan	<0.1
14	Canned	Japan	<0.2

* Sampled 5 Sept. 1959 from Willapa Bay. † Sampled 27 Jan. 1960 from Willapa Bay. ‡ Oyster stew.

Samples 2 and 3 were obtained directly from the oyster beds in Willapa Bay near Bay Center, Wash. Approximately half of the oysters produced on the West Coast are grown at Willapa Bay (*10*), which is located about 30 miles north of the mouth of the Columbia River. A sea-water sample collected over the Willapa Bay oyster beds contained 0.0002 $\mu\mu$c of Zn^{65} per milliliter. If this single sample is representative, the oysters concentrate the Zn^{65} by a factor of about 2×10^5, the same value reported by Chipman *et al.* (*11*) for nonradioactive zinc.

Although the Zn^{65} concentration in oysters from Willapa Bay is relatively high compared with that from other locations, the added exposure which an individual would receive from eating 1 pint of such oysters every week would amount to less than 10 percent of the exposure from background radiation which an individual receives continuously from natural sources. [By using the Handbook No. 69 parameters (*7*), it is estimated that a continuous intake rate of 2.2 μc/day would result in a dose of 5 rem/yr to the body—assuming that the natural background is 100 mrem/yr].

The Zn^{65} concentrations observed in the other sea foods were much lower than those found in the oyster. Although Zn^{65} was detectable in about half of these samples, the concentration in all but two was below 0.5 $\mu\mu$c/g. These two were a tuna sample (1.1 $\mu\mu$c/g) processed in Astoria, Ore., and a clam sample (2.8 $\mu\mu$c/g) obtained from Willapa Bay.

This study is being continued, with larger samples where necessary, to obtain a reasonably accurate measurement of these trace quantities of Zn^{65}. Although Zn^{65} is by far the major gamma-ray-emitting radioisotope observed in sea foods, other radioisotopes are known to be present. Cobalt-60 and phosphorus-

32 have been detected in oyster samples from Willapa Bay. Plans are under way to separate chemically and to measure the concentration of phosphorus-32, scandium-46, chromium-51, manganese-54, iron-59, cobalt-60, strontium-90, and some of the long-lived rare earths (which are present in Columbia River water) in West Coast sea foods (*12*).

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12. This work was performed for the U.S. Atomic Energy Commission under contract No. AT(45-1)-1350.

11 August 1960

Calvacin: A New Antitumor Agent

The development of a new oncostatic agent, recently named "calvacin," arose from the earlier findings of Lucas *et al.* (*1*). They reported the presence of unknown oncostatic principles in aqueous extracts of sporophores of giant puffball mushrooms, species *Calvatia gigantea*. They also observed that shake-flask cultivation of certain mycelial strains, derived from *Calvatia* sporophore tissue, led to the elaboration of an antitumor substance (or substances) in the medium.

These preliminary observations formed the basis of a more extensive program to isolate the active agent (or agents) present in the mushroom and to develop a submerged fermentation process which would lead to the development of a more convenient and reproducible source of calvacin. Active participation in the program was undertaken by the central research department of Armour and Company, the departments of horticulture and chemistry, Michigan State University, and the division of experimental chemotherapy, Sloan-Kettering Institute for Cancer Research. Isolation of calvacin from sporophores and the development of the fermentation process were the responsibility of the Armour groups. The development of new *Calvatia* strains which could provide higher yields of calvacin, and other related nutritional studies, were undertaken at Michigan State University. All test samples generated from these research efforts were submitted to the Sloan-Kettering group, who determined their antitumor activity in the standard sarcoma-180 mouse assay (*2*).

Results of more than 2 years of research have led to the isolation from sporophores, by two independent techniques, of a highly purified mucoprotein fraction with a specific S-180 activity of 300 to 500 calvacin units per milligram (*3*). The screening of several hundred mycelial cultures led to the isolation of *Calvatia gigantea* strains which provided both higher yields of calvacin and shorter fermentation periods. In addition, both laboratory (14 lit.) and pilot-plant (230 to 1200 gal) fermentation processes have been developed. Scale-up of the fermentation process to the pilot-plant stage and the subsequent recovery of calvacin from beer (on cellulose ion-exchange columns) were carried out, with results comparable to those of the laboratory process.

A purified fraction derived from sporophores of *Calvatia gigantea* (200 calvacin units per milligram) was subjected to a broad-screen tumor survey (see Table 1) and was found to possess antitumor activity against 14 of 24 various mouse, rat, and hamster tumors.

Physicochemical studies of calvacin derived from either sporophores or fermentations indicate that the products are identical or essentially similar in nature. Present knowledge of the chemical nature of calvacin indicates that it is a nondiffusible, basic mucoprotein. Calvacin is moderately heat-stable and is amenable to organic solvent precipitation or treatment with anionic and cationic cellulose ion-exchange materials. (*4*).

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Table 1. Tumor spectrum studies, in mice, rats, and hamsters, on calvacin derived from *Calvatia gigantea*. (−) No effect (tumors in treated animals developed to three-fourths, or more, the size of those in controls); (±) slight inhibition (tumors in treated animals developed from one-fourth to three-fourths the size of those in controls); (+) marked inhibition (tumors in treated animals developed to one-fourth, or less, the size of those in controls).

Tumor	Effect of calvacin (1 mg/kg/day)
Sarcoma–180	±
Sarcoma–180 (ascitic)	+
Sarcoma MA 387	+
Mammary adenocarcinoma E 0771	±
Mecca lymphosarcoma	±
Ridgeway osteogenic sarcoma	±
Bashford carcinoma 63	−
Carcinoma 1025	−
Ehrlich carcinoma	±
Ehrlich carcinoma (ascitic)	−
Lewis bladder carcinoma	±
Lewis lung carcinoma	±
Glioma 26	−
Friend virus leukemia	−
Friend virus leukemia (solid form)	±
Harding-Passey melanoma	±
Leukemia L 4946	−
Jensen rat sarcoma	−
Murphy-Sturm lymphosarcoma	−
Walker carcinosarcoma 256	±
Flexner-Jobling carcinoma	Toxic
Crabb hamster sarcoma	+
Fortner adenocarcinoma (small bowel)	−
Fortner adenocarcinoma (pancreas)	−

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3. One calvacin unit per milligram is defined as the potency of a *Calvatia* fraction that causes a 25-percent inhibition of mouse sarcoma-180 (by measurement of tumor diameter) when tested at a dosage level of 1.0 mg per mouse per day under standard conditions. The purified fractions show significant activity at 2.0 to 3.3 μg per mouse per day.
4. This investigation was supported in part by a grant from the Charles Ulrich Bay Fund, by research grant CY 3192 from the National Cancer Institute, U.S. Public Health Service, by a contract (SA–43–ph–2445) with the Cancer Chemotherapy National Service Center, National Cancer Institute, and by grants from the American Cancer Society.

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25 August 1960

Letters

(Continued from page 1858)

in the English system of units, elegantly and systematically developed the logical conclusion: the potential confusion inherent in the English system of units can be avoided by use of the metric system. (My sharp-eyed critics all noted, implicitly or explicitly, that the various definitions and revisions of the English units are always made in terms of the metric system.) As a postscript to Allen's letter, I wish to quote a relevant resolution from the recent 12th general assembly of the International Union of Geodesy and Geophysics (Helsinki, August 1960):

"The IUGG, considering international procedure concerning the use of metric units in scientific reports, strongly recommends that this practice be adopted in all papers submitted to IAGA. Thus heights of rockets and satellites should be given in kilometers instead of miles and altitudes of balloons and aircraft in meters or kilometers instead of feet."

As for Newell's ribbing on the subject of conversions, hidden in his first paragraph is some useful advice for those news media which are not concerned with accuracy and which want to "have what they say remembered." (I supposed that *Science* was concerned to have its articles be first correct and, if possible, remembered.) It is, of course, a psychological accident in the case in question that the number 9988, which cannot be justified on any technical grounds, appears acceptable, accurate and mnemonic, whereas the number 10,009, which is reasonably justifiable, appears to be either an error or a joke, and virtually demands rounding downward to 10,000. But once such a rounding has been effected, especially if it is then converted to 5 tons, the reader has lost all contact which the apparent degree of accuracy expressed in the original data. In fact, the question then arises, English or metric tons?

In general, I would recommend quoting at least the original data. If *Science* editors believe that a significant portion of *Science* readers do not comprehend the metric system, I would recommend, in this specific instance, a rendition such as: "4540 kg (approx. 10,000 lb)."

Now the witty Newell has also scored a more prevalent problem in conversions: the apparent increase in accuracy through use of conversion factors with *more* significant figures than the original data. But a word of caution to us would-be pedants: Newell happily increased four-place accuracy (4540 kg) to 13-place accuracy through use of a conversion factor with ten or more places (0.4535924277 . . . kg/lb), but unhappily he overlooked the revision (1 July 1959) of this factor. New value: 1 lb = 0.45359237 kg. One pound avoirdupois, that is.

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On Ignoring Ancient Asia

Is there not some imprecision in the first two sentences of Hutchison's article [*Science* **132**, 643 (9 Sept. 1960)]? Hutchison says: "The main interest of the ancients in the absorption of sound was an indirect one. It concerned the fabrication of bells, which, until about the 8th century, were made of beaten iron sheets riveted together." By *ancients* he certainly does not intend to include the Chinese bell founders, who, long before the 8th century A.D., cast their bells.

That the background to the vast bulk of what constitutes our "science" today lay in Europe is traditional; is it wise to continue to ignore ancient Asia? Must we continue the error of the past in regarding Europe and Asia as two separated continents?

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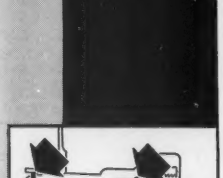

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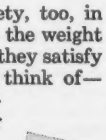

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Meetings

Esperantists

The 45th annual international congress of Esperantists was held in Brussels, Belgium, from 30 July to 6 August, 1960, under the "alta protektado" of His Majesty Baudouin, King of the Belgians. In attendance were 1930 persons from many parts of the world, representing 42 nations. At the opening session a letter of greeting from King Baudouin was read; an address was given by M. Van Acker, former Prime Minister of Belgium; and short speeches were made by official representatives of several countries and by representatives of certain agencies of some others, including the U.S., whose Voice of America uses Esperanto as well as various national languages.

A number of organizations which use Esperanto or are promoting its use for their special purposes held meetings in connection with the general congress. Among them was the Internacia Scienca Asocio Esperantista, which has been in existence more than half a century and publishes an organ of its own, as well as contributing directly, through articles by individual members, to scientific and technical journals which use mainly the national languages. All the sessions of these diverse organizations, as well as the sessions and social functions of the general congress, were carried on smoothly and expeditiously in Esperanto, without translation.

Whereas the 1959 congress (held in Warsaw) emphasized the "Zamenhof year," in commemoration of the centenary of the Polish-born author of Esperanto, whose name appears on UNESCO's list of persons whose birthdays the member nations are requested to honor, the 1960 congress urged that special effort be made during the coming year to increase the teaching of Esperanto as a school subject. It was noted, among other things, that nearly 4000 pupils are taught Esperanto in 127 schools of various grades in Yugoslavia, no doubt owing in part to the efforts of Esperantists during their "summer pedagogical week."

The congress in Brussels was preceded by the usual institute, the so-called Somera (summer) University, during which experts on various subjects lecture in Esperanto. Maximum attendance at the lectures was reported as about 700. Among the subjects treated were the following: Problems of international law relating to space travel; juristic problems relating to atomic energy; the spawning of eels and the relation of European and American eels; Belgian literature from the French point of view; English education as viewed by other countries; toponomy (the study of place names); and historical data on commercial relations between the Canary Islands and Flanders.

An invitation for the 46th congress to convene in Harrogate, England, in 1961 was presented and accepted. As for the 1965 congress, for which an invitation from Japan had been announced at the Warsaw congress, it was stated that the invitation would probably be formally repeated later, with full details, after the forthcoming meeting of the national congress of Japanese Esperantists.

IVY KELLERMAN REED
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La Jolla, California

Forthcoming Events

January

8–12. Thermoelectric Energy Conversion, symp., Dallas, Tex. (P. H. Klein, General Electric Co., Electronics Lab., Bldg. 3, Room 221, Syracuse, N.Y.)

8–13. American Acad. of Orthopedic Surgeons, Miami Beach, Fla. (J. K. Hart, 116 South Michigan Ave., Chicago 3, Ill.)

8–14. Bahamas Conf. on Hypertension, Nassau. (I. M. Wechsler, P.O. Box 1454, Nasssau)

8–14. International Conf. of Social Work, 10th, Rome. (Miss R. M. William, ICSW, 345 E. 46 St., Room 1012, New York 17)

8–15. Latin American Convention of

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Astronomy, 2nd, Lima, Peru. [A. C. Parro, Enrique Palacios 187 (359), Chorrillos (Lima), Peru]

9–11. Reliability and Quality Control, 7th natl. symp., Philadelphia, Pa. (R. L. Schwerin, ACF Electronics Div., ACF Industries, Inc., 11 Park Place, Paramus, N.J.)

9–12. White House Conf. on Aging, Washington, D.C. (Special Staff on Aging, Office of the Undersecretary, Dept. of Health, Education and Welfare, Washington 25)

9–13. Society of Automotive Engineers, annual, Detroit, Mich. (SAE, 485 Lexington Ave., New York 17)

10–11. Conference on Physics of Polymers, Bristol, England. (Organizing Secretary, Physical Soc., 1 Lowther Gardens, London, S.W.7)

16–18. American Astronautical Soc., annual, Dallas, Tex. (F. F. Martin, AAS, 304 S. Woodstock Dr., Haddonfield, N.J.)

16–19. Instrument Soc. of America, winter instrument-automation conf., St. Louis, Mo. (W. H. Kushnick, 313 Sixth Ave., Pittsburgh 22, Pa.)

22–28. Bahamas Serendipity Conf., 3rd, Nassau. (I. M. Wechsler, P.O. Box 1454, Nassau)

23–25. Institute of the Aeronautical Sciences, 29th annual, New York, N.Y. (Meetings Dept., IAS, 2 E. 64 St., New York 21)

23–26. American Meteorological Soc., 41st annual, New York, N.Y. (K. C. Spengler, AMS, 45 Beacon St., Boston 8, Mass.)

24–27. American Mathematical Soc., 67th annual, Washington, D.C. (J. W. Green, Univ. of California, Los Angeles)

24–27. Society for Industrial and Applied Mathematics, Washington, D.C. (G. Kaskey, Remington Rand Univac, 1900 W. Allegheny Ave., Philadelphia, Pa.)

24–27. Society of Plastics Engineers, 17th annual conf., Washington, D.C. (T. A. Bissell, SPE, 65 Prospect St., Stamford, Conn.)

25–27. Mathematical Assoc. of America, annual, Washington, D.C. (H. L. Alder, Dept. of Mathematics, Univ. of California, Davis)

26–27. Western Spectroscopy Conf., 8th annual, Pacific Grove, Calif. (R. C. Hawes, Applied Physics Corp., 2724 S. Peck Rd., Monrovia, Calif.)

27–28. Royal College of Physicians and Surgeons, annual, Ottawa, Ontario, Canada. (T. J. Giles, 150 Metcalfe St., Ottawa)

28–30. Control of the Mind, symp., San Francisco, Calif. (Dept. of Continuing Education in Medicine, Univ. of California Medical Center, San Francisco 22)

28–31. Infertility, sectional meeting, Intern. Fertility Assoc., Acapulco, Mexico. (M. L. Brodny, 4646 Marine Dr., Chicago 40, Ill.)

29–3. American Inst. of Electrical Engineers, winter meeting, New York, N.Y. (E. C. Day, AIEE, Technical Operations Dept., 33 W. 39 St., New York 18)

30–3. Clinical Cong. of Abdominal Surgeons, Miami Beach, Fla. (B. F. Alfano, 663 Main St., Melrose 76, Mass.)

30–4. American Library Assoc., midwinter meeting. (Mrs. F. L. Spain, New York Public Library, 20 W. 53 St., New York, N.Y.)

31–4. American Assoc. of Physic Teachers, New York, N.Y. (F. Verbrugge, 135 Main Engineering, Univ. of Minnesota, Minneapolis)

31–4. American Physical Soc., annual, New York, N.Y. (K. Darrow, APS, Columbia Univ., 116th St. and Broadway, New York)

February

1–3. Solid Propellant Rocket Conf., American Rocket Soc., Salt Lake City, Utah. (R. D. Geckler, Aerojet-General Corp., P.O. Box 1947, Sacramento, Calif.)

1–3. Winter Military Electronics Conv., 2nd, Inst. of Radio Engineers, Los Angeles, Calif. (A. N. Curtiss, IRE Business Office, 1435 S. La Cienega Blvd., Los Angeles 35)

1–4. American Physical Soc., annual, New York, N.Y. (K. K. Darrow, APS, 538 W. 120 St., New York 27)

2–4. Congress on Administration, 4th annual, Chicago, Ill. (R. E. Brown, American College of Hospital Administrators, 840 N. Lake Shore Dr., Chicago 11)

6–8. American Acad. of Allergy, 17th annual, Washington, D.C. (J. O. Kelly, 756 North Milwaukee St., Milwaukee 2, Wis.)

6–8. Geodesy in the Space Age, symp., Ohio State Univ., Columbus. (W. A. Heiskanen, Ohio State Univ., 1314 Kinnear Road, Columbus 12)

9–15. Second Allergy Conf., Nassau, Bahamas. (I. M. Wechsler, P.O. Box 1454, Nassau)

13–16. American Soc. of Heating, Refrigerating and Air-Conditioning Engineers, Chicago, Ill. (R. C. Cross, 234 Fifth Ave., New York 1)

14–15. Conference on Microdosimetry, 2nd, Rochester, N.Y. (N. Kreidl, Bausch & Lomb Optical Co., Inc., Rochester 2)

15–17. International Solid-State Circuits Conf., Philadelphia, Pa. (J. J. Suran, Bldg. 3, Room 115, General Electric Co., Electronics Park, Syracuse, N.Y.)

16–18. Biophysical Soc., annual, St. Louis, Mo. (W. Sleator, Dept. of Physiology, Washington Univ., St. Louis 10)

22–25. American Educational Research Assoc., annual, Chicago, Ill. (G. T. Buswell, 1201 16th St., NW, Washington 6)

23–25. American Orthopsychiatric Assoc., annual, New York, N.Y. (Miss M. F. Langer, 1790 Broadway, New York 19)

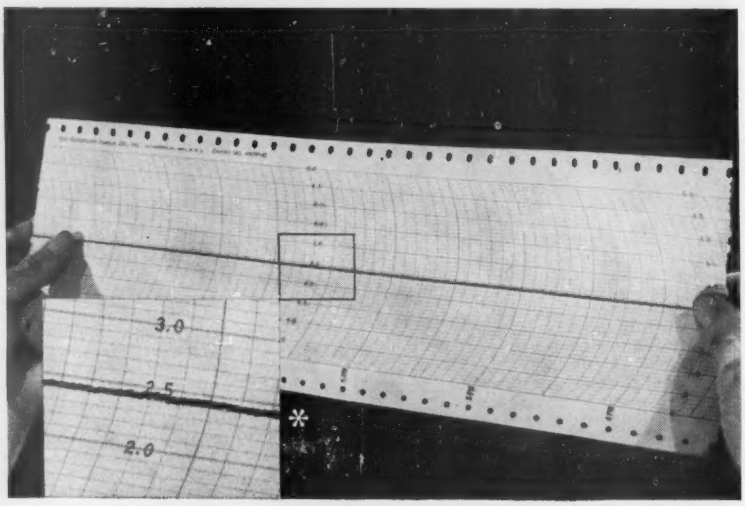
23–25. Fifteenth Annual Symp. on Fundamental Cancer Research, Houston, Tex. (Publications Dept., Univ. of Texas M.D. Anderson Hospital and Tumor Inst., Texas Medical Center, Houston 25)

26–1. American Inst. of Chemical Engineers, natl., New Orleans, La. (F. J. Van Antwerpen, AICHE, 25 W. 45 St., New York 36)

26–2. American Inst. of Mining, Metallurgical, and Petroleum Engineers, annual, St. Louis, Mo. (AIME, 29 W. 39 St., New York 18)

27–3. Conference on Analytical Chemistry and Applied Spectroscopy, 12th, Pittsburgh, Pa. (L. P. Melnich, U.S. Steel Corp., Monroeville, Pa.)

(See 16 December issue for comprehensive list)



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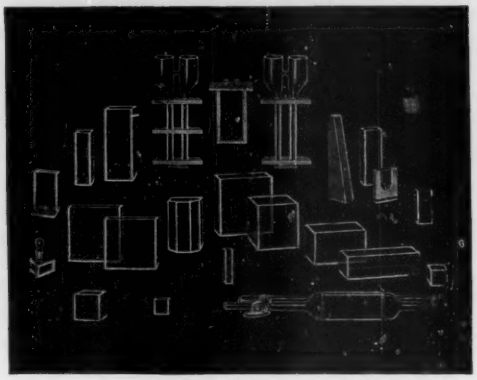

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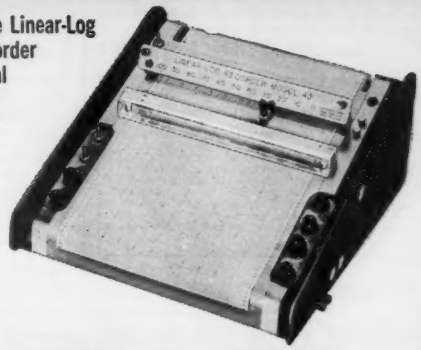

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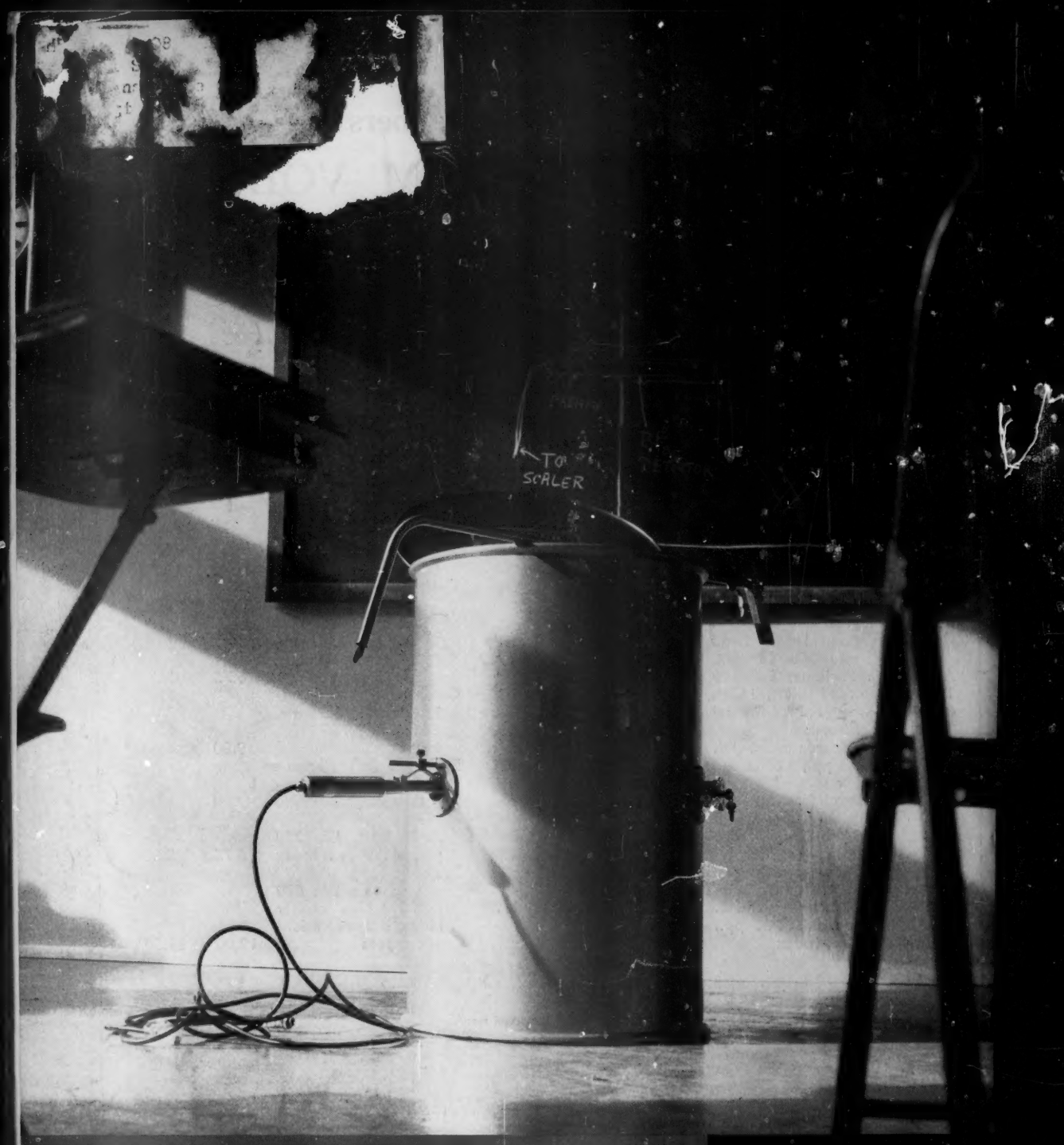

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